PROCEEDINGS

OF THE

INTERNATIONAL CONFERENCE

ON

AERIAL NAVIGATION

HELD IN CHICAGO,

AUGUST 1, 2, 3 AND 4, 1893

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CONTENTS.

Proceedings of the Conference on Aerial Navigation.

HELD IN CHICAGO, AUGUST 1, 2, 3, and 4, 1893.

THE proposal to hold an International Conference on Aerial Navigation in Chicago during the Columbian Exposition first originated with Professor A. F. Zahm, of Nôtre Dame University. He conferred with Mr. C. C. Bonney, President of the World's Congress Auxiliary, an organization under the auspices of the World's Columbian Exposition, intended to promote the meeting of various congresses; then he interested various persons in the project, and in December, 1893, a committee of organization was formed.

This committee, in consultation with President Bonney, decided to hold the Conference during the same week as the Engineering Congress, in order to secure the attendance of engineers at the discussions; and the following circular was issued:

"Not things, but men."

President, Charles C. Bonney. Treasurer, Lyman J. Gage.
Vice-President, Thomas B. Bryan. Secretary, Benjamin Butterworth.

THE WORLD'S CONGRESS AUXILIARY OF THE WORLD'S COLUMBIAN EXPOSITION OF 1893.

Department of Engineering.

GENERAL DIVISION OF AERIAL NAVIGATION.

PRELIMINARY ADDRESS OF THE WORLD'S CONGRESS COMMITTEE ON AN INTERNATIONAL CONFERENCE ON AERIAL NAVIGATION.

IN connection with the various congresses which will be held next year, under the auspices of the World's Congress Auxiliary, it is proposed to hold in Chicago, in 1893, an International Conference on Aerial Navigation, somewhat similar to that which took place in Paris during the French Exposition of 1889; the subject being one which, while it has hitherto been left chiefly in the hands of the more imaginative inventors (perhaps in consequence of the prodigious mechanical difficulties which it involves), has of later years attracted the attention of some scientific men and engineers.

OBJECTS.—The principal objects of the Conference will be

to bring about the discussion of some of the scientific problems involved; to collate the results of the latest researches; to procure an interchange of ideas, and to promote concert of action among the students of this inchoate subject.

It is proposed to invite the attendance of delegates from the various aeronautical societies of the world, and generally of persons who are interested in the scientific discussion of the subject.

TIME AND PLACE.—The Auxiliary Management has assigned the afternoons of three days for this conference, being Tuesday, Wednesday, and Thursday, August 1, 2, and 3, 1893. The opening session will take place at 2.30 P.M., on Tuesday, August 1, in one of the halls of the "World's Congress Art Palace," now being built on the Lake Front Park, at the foot of Adams Street, in Chicago. The sessions upon the two subsequent days will also take place in the afternoon, and may either, like the first, be joint sessions, or the members in attendance may divide into two sections (A and B) as may be decided hereafter.

TOPICS SELECTED.—The topics selected for papers and discussions are as follows:

I.—SCIENTIFIC PRINCIPLES—JOINT SESSION.

1. Resistance and supporting power of air, including results of recent experiments; formulas for the resistance of balloons or flying machines, etc.

2. Best forms of aerial propellers, including results of experiments with screws, wings or other forms; their efficiency and the power required.

3. Motors for aeronautical purposes, whether steam, gaseous, electric, explosive, etc.; their effectiveness, safety and weight per H.P.

4. Materials for aeronautical construction, whether for balloons or flying machines; including the strength and weight of fabrics, metals, woods, etc.

5. Best structural forms for aeronautical constructions, so as to combine strength and lightness; to offer the least resistance to progression, and to alight safely.

6. Behavior of air currents, including observations at various altitudes; the prevalence, the direction, the trend and the force of winds, etc.

II.—AVIATION—SECTION A.

1. Observations and experiments on the flight of birds, including their methods of rising, gliding, alighting, etc.; measurements of power exerted and of velocities.

2. Theories regarding the soaring and sailing of birds. It is now generally admitted that birds utilize the wind in soaring, but no satisfactory explanation of the performance has been given.

3. Various types of proposed flying machines, their advantages and defects, the power required, their safety; differences between natural and artificial wings, etc.

4. Equilibrium of flying machines, including the best means of securing safety with wings, screws, aeroplanes, etc., in rising, sailing, and alighting.

5. Novel experiments in aviation and their results, either with power machines, dirigible parachutes, gliding or soaring devices, models, etc.

6. Experiments with kites; results of different forms as to stability, sustaining power, height attained, behavior, etc. A history of kites.

III.—Ballooning—Section B.

1. Construction of balloons, choice of fabrics, laying out, cutting and sewing, varnishes, nets, cars, valves, anchors, guide ropes, parachutes, etc.

2. Inflation of balloons; hydrogen, coal gas, natural gas, hot air, etc.; their generation, cost, and management during inflation.

3. Navigable and war balloons, past experiments and results; the present status; the resistance, propellers, motors, speeds, etc.

4. Manœuvring of balloons, ascending and descending, with least expenditure of ballast or gas; utilizing wind currents, determining altitudes, etc.

5. Observations in balloons, meteorological, photographic, topographical, military, naval, planimetric, etc.; various instruments required.

6. Proposed improvements in balloons, as to forms of minimum resistance, increased strength and stiffness; with calculations of power required and lifted.

The Organizing Committee may arrange upon application for the introduction and discussion of topics not enumerated in the above list.

Proceedings.—It is intended to introduce each of these topics by the reading of one or more papers thereon, to serve as a basis for discussion, and to draw out further information. These introductory papers will be obtained both by solicitation of the Organizing Committee and by voluntary contribution. They need not be long nor very exhaustive, but decided preference will be given to those stating the results of actual experiments; as facts and positive knowledge are deemed more instructive than theories or projects. It is expected that some of these papers will be printed and distributed in advance, in which case it will be preferred to receive discussions thereon in writing.

No paper will be read unless it has previously been approved by the Committee of Organization. The management of the World's Congress Auxiliary will appoint the officers to preside over the various sessions, and these officers will arrange the order of the proceedings, call up in their turn the various papers, and the speakers whom the Organizing Committee may have selected to discuss them. Papers previously printed will generally be presented by abstract, so that discussion may follow without loss of time. Persons desiring to join in the

discussions will be expected to give previous notice, and the remarks of speakers will generally be confined to fifteen minutes, and to not more than two speeches upon the same subject. It is preferred that speakers shall subsequently furnish a *résumé* of their remarks in writing, failing which the stenographer's notes will be edited by the committee.

Stenographers will be in attendance, and interpreters will be provided when previous notice has been given of remarks to be made in other than the English language.

It is expected that a separate room will be provided, in which to exhibit, on approval of the committee, small models or interesting experiments during the intermissions between the meetings. Should circumstances warrant, one or two additional sessions may be held.

CARDS OF ADMISSION.—Personal cards of admission to the Conference will be issued in advance by the Secretary of the Organizing Committee upon application to him, appproval by the Committee, and the payment of a contribution of $3 to the publication fund. These cards will entitle the holder to attend the Conference and to receive all of its subsequent publications.

PUBLICATIONS.—The Committee of Organization will decide how much of the papers and proceedings shall be printed, and will cause the same to be edited. Such of the papers as may be printed in advance will be mailed to the holders of cards of admission who may request it, and designate the particular topic or topics which they desire to discuss. Written discussions thereon should be forwarded to the Secretary in advance of the Conference, and after its close all such papers and discussions as may be printed shall be mailed to the members thereof.

ORGANIZATION.—The President of the World's Congress Auxiliary has appointed a local committee to organize the affairs of the proposed Conference. It is to be assisted by an Advisory Council consisting of the leading scientific authorities on the subject throughout the world. Persons desiring to secure cards of admission or to contribute to the papers or discussions are requested to advise the secretary at an early day, stating in the latter case what is the class of researches or experiments which they have made, and on what topics they desire to receive advance papers.

All communications should be addressed to Professor A. F. Zahm, Secretary, Nôtre Dame, Ind.

O. CHANUTE, *Chairman.*
A. F. ZAHM, *Secretary.*

ELISHA GRAY, LL.D.,	E. L. CORTHELL,
H. S. CARHART,	R. W. HUNT,
S. W. STRATTON,	D. J. WHITTEMORE,
IRA O. BAKER,	J. W. CLOUD,
JOHN GUERIN,	B. J. ARNOLD,
B. E. SUNNY,	W. N. RUMELY.

Committee of the World's Congress Auxiliary on Aerial Navigation.

WORLD'S CONGRESS HEADQUARTERS,
CHICAGO, ILL., December, 1892.

A large number of letters were written to those persons in various parts of the world who were known to be experts or students of the several topics selected, and the effort was made to secure at least two papers by competent writers upon each of the topics, so as to present two points of view for discussion.

Favorable responses were at first somewhat slow in coming, so that only a few papers could be manifolded in typewriting, and none were printed; but toward the last of July, 1893, papers came in abundance, and were of a high order of merit.

Cordial letters of co-operation were also received from the British Aeronautical Society, the Aerial Navigation Society of France, the Aviation Society of Munich, the Imperial Aeronautical Society of Russia, and the Aviation Society of Vienna.

A meeting of the Organizing Committee was held, to decide upon what papers should be submitted to the Conference. A few were rejected altogether, as being imperfect, or presenting untried projects, and some papers it was decided to present but not to print afterward, as not possessing sufficient interest for permanent preservation.

The programme as printed comprised forty-seven papers, but this included contributions from several persons who had treated more than one of the topics selected; notably the paper of Mr. G. Crossland Taylor, and the treatise on the problem of aerial navigation of Mr. C. W. Hastings, the various parts or chapters of which were presented in the programme under the appropriate topic. It has been thought best not to follow this division in publishing the proceedings, but to print the papers as originally received.

It was, of course, impracticable to read all these papers in full. Some were presented *in extenso*, and some were given in abstract, in order that discussion might follow.

The Conference took place in the Memorial Art Palace, in Chicago, August 1, 2, 3, and 4, 1893, the session upon the first day (Scientific Principles) being presided over by Mr. O. Chanute, Chairman of the Organizing Committee; the second session (Aviation) being presided over by Dr. Thurston, Director of Sibley College, Cornell University; the third session (Ballooning) was presided over by Colonel W. R. King, of the U. S. Army; while the fourth session was a supplemental one, in which the various topics presented were further discussed.

The attendance at each session comprised about one hundred persons, who seemed to take great interest in the proceedings, and the discussions brought out several investigators who had been studying the subject or trying interesting experiments without making it publicly known.

OPENING ADDRESS.

BY O. CHANUTE.

It is well to recognize from the beginning that we have met here for a conference upon an unusual subject; one in which commercial success is not yet to be discerned, and in which

the general public, not knowing of the progress really accomplished, has little interest and still less confidence.

The fascinating because unsolved problem of aerial navigation has hitherto been associated with failure. Its students have generally been considered as eccentric—to speak plainly, as "cranks;" and yet a measurable success is now probably in sight with balloons—a success measurable so far that we can already say that it will probably not be a commercial one; while as to flying machines proper, which promise high speeds, we can say that the elements of an eventual success, the commercial uses of which are not as yet very clear, have gradually accumulated during the past half century.

The truth of these assertions, which will be justified further on, seems to indicate that it is not unreasonable for us as engineers, as mechanicians and as investigators, to meet together here in order to discuss some of the scientific principles involved, and to interchange our knowledge and ideas.

The present is, I believe, the third international conference upon aerial navigation. The second took place in Paris in 1889, and a fourth is projected to take place in that city during the Exposition of 1900.

The conference of 1889 undoubtedly forwarded the possible solution of the problem by making the public aware that a number of sane men were studying it in various parts of the world, by making these men acquainted with each other's labors, and by disseminating information concerning the scientific principles involved, the mechanical difficulties to be surmounted, and the practical details of aerial construction generally. Probably as a consequence of this, very considerable advance has been made during the last four years, as will be indicated hereafter, and a number of promising proposals are now in progress of experiment and development.

We may fairly expect similar results to follow from the present conference. We may hope to collate here considerable knowledge concerning the scientific principles involved, to gain information concerning the latest researches, and to establish some concert of action.

Indeed, we shall begin our proceedings with the presentation of a paper by Professor Langley conveying what may almost be said to be the exposition of a new natural law, hitherto but dimly suspected, which seems to hold out promises of important consequence.

We neither expect nor desire the presentation here of new projects for navigable balloons or for flying machines. We have endeavored to secure instead the statement of general principles and of the results of actual experiments, as facts and positive knowledge are deemed more instructive than projects or speculations.

Success, when it comes, is likely to be reached through a process of gradual evolution and improvement; and the most that we can hope to accomplish at present is to gain such knowledge of the general elements of the problem as to enable

us to judge of the probable value of future proposals, both as mechanical or as commercial enterprises.

More important still, we may perhaps help to enlighten a number of worthy but ill-equipped inventors who are retrying old experiments, with no proper understanding of the enormous mechanical difficulties involved.

As a preliminary to our proceedings, it will probably be interesting to you to have a brief survey of what has already been accomplished, both with balloons and with flying machines, and of the advance which has been achieved since 1889.

As regards navigable balloons, the latest reliable information is probably contained in an interesting and carefully prepared paper, read by Mr. Soreau, C.E., before the French Society of Civil Engineers in February last, and discussed at the April meeting of that society.

You know that it has been abundantly proved that elongated balloons of large size can be made sufficiently stiff by internal gas pressure to stand driving at low velocities. The best speed hitherto obtained in any public trials has been 14 miles per hour, which is quite insufficient to stem the wind upon any but rare occasions. This speed was achieved by Commandant Renard, of the French Military Aeronautical Department, in 1885. The balloon was 165 ft. long and 27½ ft. in diameter, carrying an electric motor weighing 1,174 lbs., which developed 9 H.P. The motor, therefore, weighed 130 lbs. per horse power.

Now, the French technical papers announce, and Mr. Soreau confirms, that during the past winter Commandant Renard has been constructing a new war balloon 230 ft. long and 42½ ft. in diameter, which is provided with a new motor said to be of 45 H P., and to weigh with 10 hours' supplies, between 2,640 and 3,080 lbs., or at the rate of about 66 lbs. per horse power. With this apparatus, and with a screw some 30 ft. in diameter, it is said that Commandant Renard expects to obtain a speed of 24½ miles per hour, and that this will enable him, for about three-quarters of the days in the year, to stem the winds that blow.

Granting that the statements made about the motor are true (and there is nothing improbable about them, as we shall presently see), and also that the motor (the details of which are kept secret) shall not break down upon trial, I see no good reason to doubt the attainment of the speed estimated ; and we may learn any day that it has been performed, although it is understood that the French authorities are maintaining such secrecy as they can concerning this new war engine.

But the Germans also, as well as several other European nations, are said to be in possession of navigable war balloons ; and should war break out in Europe (which Heaven avert !) we might be very soon made aware of the fact that speeds of 25 miles an hour are practicable.

I have no doubt about it myself ; but the attainment of this moderate speed requires very large, and, therefore, very costly balloons, which carry very few passengers ; and it is clear that

while such craft may be justified by the exigencies of war, they cannot compete commercially with existing modes of transportation.

The difficulty with navigable balloons is that they must be of very great dimensions for even moderate speeds and very light useful loads. As the cubic contents of the gas bag increase at a higher ratio than the surface of its envelope, the relative lifting power increases with the size, and therefore more powerful motors can be taken up and more speed obtained; but we soon reach the limits of practicability. The new French war balloon is 230 ft. long (as large as a lake steamer), and it will carry but three or four passengers at 25 miles an hour, so that it is difficult to conceive how, if they be made of sufficient size to carry even a score of passengers, such enormous and frail craft can be handled, housed, or operated without peril of casualty or disaster.

The conditions as to resistances, lifting power, propellers, and motors are now pretty well known; the speeds can be calculated with approximate accuracy; and while improvement can doubtless be achieved in the energy of the motor, in the efficiency of the screw, and especially in the form of the navigable balloon to diminish the resistance, it may be affirmed with confidence that railway express train speeds cannot be attained with balloons of practicable dimensions. They may be used for war purposes or for exploration, but while we may say that the balloon problem is approximately solved, we may also say that the solution does not promise to become a commercial success, or to yield a large money reward to inventors.

With artificial flying machines proper, should a practical one eventually be developed, very much higher speeds may be expected. The pigeon flies at 60 miles an hour, the duck at 90, the swallow at 125, and the marten is said to flash through the air at something like 200 miles an hour. Professor Langley has lately shown that, within certain limits, high speeds through the air will be more economical of power than low speeds, and recent advance in light steam-engines seems to have reduced them to a less weight per horse power than is generally thought to obtain with the motor arrangement of birds. It seems, therefore, not unreasonable to entertain the hope that man may eventually achieve a mechanical success (if not a commercial one) in the attempt to compass a mode of transportation which so strongly appeals to the imagination, and that it may result in greater speeds than pertain to our present journeyings.

The mechanical difficulties in obtaining safe support from so intangible a fluid as air are, however, so great that men would long ago have given up the attempt if it had not been for the birds. But, then, there are the birds; and some of them at least—the sailing birds, concerning which you will hear something in some of the papers to be read here—seem to be able to soar indefinitely upon the wind with no muscular effort whatever, so that the argument which has been made

that man cannot hope to float his greater weight than theirs upon the air would seem not to be well founded.

But, as already stated, the mechanical difficulties are very great, and it is not surprising that they should have deterred many men competent to advance the solution of the problem from considering it at all, and that it should have mainly been left in the hands of the more imaginative and ill-informed inventors, who, with imperfect knowledge of the elements of the problem, believe that success is to be achieved through a single happy thought.

It is a mistake to suppose that the problem of aviation is a single problem. In point of fact, it involves many problems, each to be separately solved, and these solutions then to be combined. These problems pertain to the motor, to the propelling instrument, to the form, extent, texture, and construction of the sustaining surfaces, to the maintenance of the equipoise, to the methods of getting under way, of steering the apparatus in the air, and of alighting safely. They each constitute one problem, involving one or more solutions, to be subsequently combined; and these are the elements of success already alluded to as having gradually accumulated, which I propose to pass in review, more particularly to appreciate what has been accomplished since 1889.

First, as to air resistances and the support to be obtained from its inertia, we have had the magnificent labors of Professor Langley, published in 1891, showing, by careful experiments that something like 200 lbs. can be sustained in the air by the exertion of 1 H.P. One half of this weight has already been supported per horse power in some experimental machines.

Then, as to the motor: Mr. Maxim has recently announced that he has constructed two steam-engines of 300 H.P. which, with the engine proper, the boilers, pumps, generators, condensers, and the weight of water in the complete circulation, weigh but 8 lbs. to the horse power. With respect to the propelling instrument, Mr. Maxim has, since 1889, made a great many experiments with aerial screws. He finds, like Commandant Renard before him, that some forms are very much more effective than others, so that the coefficient of efficiency, which was less than 35 per cent. in the earlier aerial screws, may now be said to be at least double this amount.

On the other hand, Mr. Hargrave, who now has built and experimented some 18 different flying machines, all of which fly, says that he has obtained equal propulsive effects from screws and from beating wings, although he rather prefers the latter. A paper from him, giving the results of his latest experiments and describing his steam-engine and boiler, which weigh only 10.7 lbs. per horse power, will be submitted to this meeting.

As to the best form, extent, texture, and construction of sustaining surfaces, there is yet considerable uncertainty; but there will be submitted here two papers upon materials of aeronautical construction—one by Professor Thurston and the

other by Mr. G. Crossland Taylor—both of which are well calculated to advance knowledge on this subject; while the experiments of Mr. Phillips in England a few months ago have shown that with peculiarly shaped blades of wood about 72 lbs. per horse power can be supported in the air.

The equipoise is, in my own judgment, one of the most important problems yet to be solved in aviation. No success is to be hoped for unless the apparatus is stable and safe in the air—safe in starting, in sailing, and in alighting. Three-quarters at least of past failures can directly be traced to lack of equilibrium. This problem seems to be in process of solution; and I may mention in this connection that during the summers of 1891 and 1892 M. Lilienthal, of Berlin, has been gliding downward through the air "almost every Sunday, and sometimes on week days," upon an aeroplane with which he expects eventually to imitate the soaring of the birds, when he has learned to manage it safely.

Several of the papers to be read here propose various methods for first acquiring this necessary skill, for first learning to fly under safe conditions before venturing to launch forth in the air. This bird-science seems to be the first requisite, for safety is indispensable; and it may not be secured in free air until skill has been acquired in handling a machine.

The problems of starting up into the air, of steering, and of alighting safely upon the ground cannot yet be said to be in process of solution. Various methods have been proposed for getting under way, the principal of which have been to gain speed upon the ground or to get a lifting action from rotating screws; but neither has as yet been practically demonstrated as quite practical upon a working scale.

For the purpose of steering it has generally been proposed to employ two rudders, one vertical and one horizontal; but it yet remains to be known whether they will prove quite effective under the varying circumstances of flight.

The alighting upon the ground is likely to prove the most difficult and dangerous of the problems to be solved. It has been much too little considered by would-be inventors and patentees of flying machines, and it may long prove a bar to the success of such apparatus, for nothing but direct experiment, and that of a perilous kind, will determine how this operation can be successfully performed.

I hope, however, that you will agree with me that some of the elements of success have gradually been accumulating, and that there has been real, substantial advance within the last few years. There is still much to be done; but a number of experimenters have each been working on one or more of the several problems involved, and they have made it more easy for others to forward the general solution still further.

From this brief review of recent progress it would appear less unreasonable than it seemed a few years ago to hope for eventual success in navigating the air, and it may now be reasonably prudent to experiment upon a small scale, particularly if the inventor does so at his own expense; for the chances of

commercial success seem still too distant to invite others to engage in the actual building of a flying machine unless they do it with the understanding that they may perhaps lose their money. This is the course which has thus far been followed by the three or four experimenters who now seem in the lead, and it may not be long before they achieve such success as fairly to warrant them in proceeding to the construction of a full-sized machine.

In any event, without concerning ourselves overmuch with the possible commercial uses of such apparatus, we may hope here to advance knowledge upon this interesting problem, and to be of service to those ingenious men who are seeking for its mechanical solution.

ON THE PROBLEM OF AERIAL NAVIGATION.

BY THE LATE C. W. HASTINGS.

THE late C. W. Hastings was a young civil engineer who was much interested in the problem of aerial navigation. He was for a time an assistant to Mr. O. Chanute, from whom, as he expressed it, "he learned much that he knew on the subject." He studied all the publications which were accessible, and made some investigations of his own, more particularly upon the aerial propeller, which will be found to be quite fully discussed in the following pages.

Finding that his health was failing, he devoted the last few months of his life to preparing the present essay.

He knew that he was dying from an incurable disease of the heart, and he bore his sufferings with a serenity and cheerful patience which touched his friends to the quick, devoting his remaining feeble strength to revising his work for publication.

He died, at the age of 33 years, in October, 1892, bequeathing this essay, which was not quite revised to his liking, to his friend, Mr. W. H. Breithaupt, who has edited it and contributed it to the Conference on Aerial Navigation in Chicago, where it was presented in abstract under the heads of the various topics selected. It is here published as originally arranged by Mr. Hastings, who thus contributes, even after his death, to the advancement of knowledge.—ED.

INTRODUCTORY.

To those who have given the subject of aerial navigation but little attention, it will appear that gravity is the insurmountable difficulty which prevents its achievement; gravity is so evident an obstacle, and so evidently a great one, that very many people will readily believe that with gravity overcome, aerial navigation should be quickly accomplished.

That gravity has been overcome is too well known, and yet aerial navigation, except in an experimental and unsatisfactory way, has not been accomplished. The overcoming of the great obstacle, combined with the almost complete failure to reach the desired end, has resulted in creating the very reasonable and almost universal belief, that the problem of aerial navigation is impossible of solution. Scientific men, however, make an exception in this almost universal skepticism, and nearly all of them are willing to admit that when sufficient progress shall have been made in mechanical science, true aerial navigation will be possible and will be accomplished.

Transit through the air, when accomplished, must be an improvement in some particular upon the present methods of transportation, or it will be worthless. The aerial craft either must carry heavy cargoes for less money than present systems cost, or it must carry cargoes at higher speeds than present systems are capable of attaining, or transit must be effected to points which it is impossible to reach by other systems, or transit in such aerial vessels must possess features of enjoyment which other methods of travel do not have.

Aerial navigation, as at present known, has none of these preferences, save possibly the last, and its expense is so great as to prohibit indulgence in it. The successful aerial craft must be commercially successful in some of the ways mentioned; for a craft which would only navigate the air in time of war, for military or naval purposes, would hardly be considered to have solved the problem in a much more satisfactory way than it has already been solved.

If the problem of aerial navigation is to be discussed by engineers, it must be discussed as other engineering problems are. There must be generally accepted physical laws, or empirical formulæ, and there must be reliable experimental data, and the whole must be treated mathematically. An attempt will be made to do this in the following pages, but owing to the fact that the subject has received so little attention from scientists, the experiments that can be depended upon are few, and the empirical formulæ will certainly need modification in the future. There are therefore many places in these pages where an apparent conclusion is reached, but this apparent conclusion is only perfunctory and may be greatly changed. At the same time there are places where the conclusion reached is final, for it is based upon absolute physical laws, and not upon empirical formulæ, or meager experimental data. The reader will have no difficulty in determining one kind of conclusions from the other.

If the author repeats, and without credit, statements made by others, he does it unintentionally. Credit has been given wherever it has been thought to be due.

No attempt has been made to indicate any particular form of apparatus for aerial navigation; nor have any suggestions been made regarding experiments which it may be necessary or desirable to try. This policy is not pursued with the desire to conceal anything, but should this indication be attempted,

it would at once be assumed that the writer had tried to solve the problem of aerial navigation, and criticism would be based upon that assumption. The paper claims only to be an attempt to indicate the road over which the successful inventor of aerial navigation must travel. Should this be clearly understood by engineers; if they are made aware of just what knowledge is required in order to accomplish aerial navigation, it is the belief of the writer that that knowledge will eventually be forthcoming.

BALLOON FLIGHT.

Gravity being the greatest apparent obstacle to flight, it is but natural to suppose that with this great obstacle removed, aerial navigation should soon be accomplished. It is now over a hundred years since the invention of the balloon* overcame the attraction of gravitation in so great a measure that man was enabled to rise vertically to an indefinite distance. Nevertheless aerial navigation has not yet become a commercial success, save as some curiosities are commercial successes.

Since the balloon certainly overcomes gravity, and can be designed to raise any weight, most of the money which has been spent up to the present time upon aerial navigation has been spent upon balloons, and upon attempts to make them navigable, and since such machines have not as yet come into general use, it is plain that they are so far commercially impracticable.

The navigable balloon consists of a cylindrical gas bag with pointed ends; it is driven through the air in the direction of the longer axis, which is horizontal. The shape is adopted so that the resistance of the air may be as small as possible, and motion is obtained from the thrust of the screw, which is turned by any suitable motive power. Steam, electricity and manual power have been used as motors.

Four such balloons have been manufactured, the most successful one being that made under the auspices of the War Department of the French Government. This balloon has made seven recorded trips, achieved a speed of 14 miles per hour, and on five of the seven occasions was able to return to the starting-point. None of the three other so-called navigable balloons deserves the name, for none of them has ever been able to return to its starting-point, and none of them was ever tried but once.

These facts are rather discouraging to those who hope to achieve aerial navigation through the means of a navigable balloon. If the French Government by the use of more money than the most enthusiastic company of promoters could hope to secure, and with, presumably, the best ability that it was possible to obtain, aided also by the fact that Frenchmen possess almost exclusively all experience in navigable balloons,

* Stephen Montgolfier made the first successful balloon ascent in June, 1783.

and that they practise general ballooning to a far greater extent than other people; if the French Government with these advantages, and with several years of diligent trial, was unable to achieve more than the very moderate success stated, it would seem as if it were useless for others to attempt to solve the problem in this way, and without government aid. But there are much better reasons than this why the navigable balloon cannot be made commercially successful.

It has been shown that by merely doubling the size of the French War balloon, its speed could be increased from 14 miles per hour to 25 miles per hour. The reason is easy to see. The force which a body has to overcome in passing through a fluid is due to the resistance of that fluid, and if the body retains the same relative shape, but only increases in size, then the resistance for the same speed will increase as the area of the greatest cross-section. If the balloon be doubled in all of its dimensions it will hold eight times the amount of gas that it did at first, and will possess eight times the lifting power; but the area of its greatest cross-section will only be increased fourfold. If the motor weighs a stated amount per horse power, then it is seen that the balloon will carry eight times the power that it did at first, but that it will meet only four times the resistance. Its proportionate power is therefore twice as great as that of the smaller balloon, it is in fact twice as powerful, and had the dimensions been multiplied by three, it would have been three times as powerful.

The double-sized balloon, however, would not travel through the air at twice the speed of the smaller-sized apparatus, for the speed of a vessel moving through a perfect fluid will only vary as the cube root of the power consumed, so that the double-sized balloon would only travel

$$\sqrt[3]{2} = 1.259,$$

say, 25 per cent. faster than the smaller vessel, if the increase in all the parts, size of operators, etc., were precisely the same. As a matter of fact, however, there are so many things in the double-sized balloon, which would be of precisely the same weight as in the smaller apparatus, that the power could be increased much more than eight times, with the result that a speed of 25 miles per hour could be obtained instead of 14×1.259, 17.5 miles per hour as theory would indicate.

Had the dimensions been multiplied by three instead of by two, the possible increase in speed, as shown by theory, would have been

$$\sqrt[3]{3} = 1.44 = 44 \text{ per cent.}$$

Theory therefore leads us to expect that the increase in speed will be equal to the cube root of the increase in the dimensions. This principle has been known for some time, and has not only been known, but has been the foundation for some elaborate calculations and extensive patents which were taken

out in 1885 by the late Mr. E. Falconnet, member of the American Society of Civil Engineers.

The balloon built by the French Government was 165 ft. long and 27¼ ft. in diameter; the cargo inclusive of the aeronauts weighed 780 lbs., and the speed was 14 miles per hour. The navigable balloon of double the size would carry 1,500 lbs., and the speed would be 25 miles per hour. The voyages in both cases would be limited to a few hours. The smallest apparatus proposed by Mr. Falconnet was about the size of the latter, and the "practical" machine, which he believed would be most used, was to be about a quarter of a mile long and 200 ft. in diameter.

A general consideration, which the most casual investigator would grant, must show him that the small cargoes and low speeds, of which the smaller machines are only capable, would make them commercial failures. The large forms of apparatus which were proposed by Mr. Falconnet could hardly have been kept under cover, and a wind such as occurs in almost any part of the world, at some time in the year, would inevitably demolish such a frail structure of such vast expanse as a balloon a quarter of a mile long and 200 ft. in diameter.

There is one element in aerial navigation by the use of balloons that has not been touched upon by writers on the subject, and that is the cost of maintenance. This omission can be accounted for by the fact that there has been no maintenance. Of the four navigable balloons constructed three have been tried but once, and then abandoned; the fourth was under the control of the French Government, and was the subject of many experiments and changes, while it is probable that reliable data of this nature could not be obtained, even if it has been thought desirable to keep the record.

It is evident that if the navigable balloon is to be kept in "commission" as many hours in the day and as many days in the year as is the steamboat or locomotive, the cost of maintenance will be ruinous. The income from a navigable balloon must therefore be very considerable in order to meet its maintenance. The character of the material of which it is constructed is so fragile, that the apparatus is extremely liable to accident, and, as compared with wood or iron, it is very shortlived; those capitalists, therefore, who may venture their money in a navigable balloon project, should calculate on large rates of profit to compensate them for the risks of the business, and in any case should expect to get their money back in a very few months or not at all.

Those who desire to still further investigate navigable balloons will find the subject succinctly and quite completely treated in a lecture by O. Chanute, C.E., before the students of Sibley College, Cornell University, published by the *Railroad and Engineering Journal*, New York.

DYNAMIC FLIGHT.

Since balloon or static flight can probably never be a commercial success, there remains only the other kind to be con-

sidered—that is to say, dynamic flight. This is at present practised by living creatures which weigh at most only 50 lbs., and imitated by some toys of about as many ounces. No flying creature exists that approximates the weight of any flying machine which would be termed successful.

As the above facts are favorite and very relevant arguments which are brought by those who believe that aerial navigation for man will never be more than a dream, it may be paramount to inquire why the flying creatures are all so small. Nature has probably developed as large a walking animal as the laws of mathematics and of the strength of material will permit, and it also seems reasonable to believe that the whale is as large an organism as the laws which govern physical structures will allow.

Since the mammoth and the mastodon were certainly smaller than the vehicles which man has constructed to navigate the land, and since the whale is not the largest body which can navigate the water, it seems reasonable to think that the eagle, albatross, or condor are not the largest bodies which can navigate the air by dynamic flight.

To any who have paid the least attention to flight, it must be evident that there are two kinds of dynamic flight—namely, beating flight and sailing flight. In the former kind the animal seems to be in many cases sustained by the wings pressing directly downward upon the air, and producing an upward reaction equal to the weight, by means of a direct downward thrust. In this way the smallest of flying animals, insects, and humming-birds may keep their bodies motionless in calm air, and even fly backward as well as forward, though they do the former but slowly. As we investigate the flight of the larger animals we find that none of them ever fly backward, and that the pigeon is about as large a bird as can raise itself vertically through calm air; it only does this with an evident effort and for short distances.

Larger birds than the pigeon are only able to rise vertically when under some extraordinary excitement, and the largest of all are unable to rise vertically at all. This shows that the action of the wing in large birds is not vertical, and as we watch them we see that their efforts are made to obtain not vertical, but horizontal motion. When the large bird rises from the ground he will do so by facing the wind and running against it; if he launches himself from a perch, it is never horizontally or upward, but always downward. If the hunter gets to the windward of a flock of wild turkeys, he can capture them, for the birds are unable to rise with the wind, but if he is to the leeward of them, they will spread their wings and easily escape. If the condor can be induced by bait to enter a small enclosure which is a little larger than his body and with its sides a little higher than his head, the bird will be unable to escape, for he cannot rise vertically.

As a very general statement, then, it may be said that large flying animals fly by sailing, and that small flying animals fly by beating their wings. To be sure there are many excep-

tions to this rule, for there are myriads of forms in nature; the butterfly will sail in an unsteady manner for a short distance, and the wild goose is an exceedingly strong flapping flyer, but the statement is quite true in a general way, and is truer in a definite way than most people suppose. The reason must, of course, be that it is easier for the small animal to fly by beating than it is by sailing, and that it is easier for the large flying animal to fly by sailing than by beating. They will both fly in the manner which consumes the least power.

Theoretically the weight of any motor will vary as the cube of its dimensions, while the power which it can exert will vary as the square of those dimensions. If we have two steam-engines precisely similar, differing only in size, and running at the same piston speed and boiler pressure, the area of the piston, and therefore the power developed, will vary as the square of the dimensions, while the weight will vary as the cube of those dimensions. Now this statement seems absurd to the practical engine builder, for he knows that as a matter of fact the weight of an engine per horse power will decrease as the size is increased. The truth is, that no two engines have been made exactly alike except in size, and that also when the size is greatly decreased the internal friction becomes such a proportion of the load that the operation of this law is scarcely perceived.

In nature, however, the case is different. Here the internal friction is negligable, and the difference between the largest and the smallest of motors is so very great that the operation of the law can be observed. The contractile power of a muscle per square inch of section is about the same whether it be taken from a large or from a small animal. It is about 20 lbs. per square in. The rapidity of contraction is perhaps no greater in the muscle of the rapid-flying bird than in the rapid land animal. At any rate, the difference is not remarkable. It is seen, therefore, that the energy of the muscle will increase with the square of the dimensions, while the weight of that muscle will increase with the cube of those dimensions. It is for this reason that the ant can lift another ant with ease, that the man lifts his fellow-man only with difficulty, and the elephant is unable to lift its fellow at all.

Now while the small animal can exert greater energy in proportion to its weight than the large animal, and is therefore better adapted to beating flight, which, as has been observed, requires more power than sailing flight, it is not so well adapted for sailing flight. Sailing flight requires a considerable horizontal velocity, and this means a considerable opposing force due to the resistance of the air.

In order that the body shall meet with as little resistance as possible, the sectional area of the greatest cross-section should be as small as possible. This sectional area will increase with the square of the dimensions, while the weight or mass will increase with the cube of the dimensions. Sailing flight undoubtedly requires mass, momentum, and this mass or momentum decreases much faster than the sectional area when the

dimensions decrease. There will be a point in the decrease in size where the mass will be so small in comparison with the resistance created by the sectional area of the greatest cross-section, that sailing flight will be impossible ; just as a speck of dust is the sport of every zephyr, while the huge stone, although of the same specific gravity, will withstand a hurricane.

Since the resistance increases much slower than does the momentum, as the size increases, we may expect that the larger flying animals will be of more rapid flight than the smaller, and as a general rule, with the numerous exceptions due to the numerous forms which nature has developed, this also is true.

Referring to the decrease in the proportional strength of the muscle with its increase in size, it is seen that there must be a point in increase of size, where the flying animal would be unable to raise itself vertically from the ground, just as there is a point in the increase of size where the quadruped would be unable to travel by walking. There does not, however, seem to be any such mathematical reason to limit the size of the animal which could travel by swimming ; and since, for the same speed, the resistance met by a body in moving through the air will increase as the square of the dimensions, and since the power of the living motor will also increase as the square of the dimensions, it would seem as if there would be no limit indicated by mathematics to the increase in size of the creature which would fly by sailing. This, however, is quite misleading, for the power required for support must be considered in discussing aerial navigation, while in marine navigation no such power is required.

The theory of the power increasing as the square of the dimensions of the motor has only been applied to the living machine. It applies there, as already stated, because the motors are similar, and because the internal friction is small ; the operation of the law can also be observed because the motors vary so greatly in size. In discussing balloon flight we assumed that the power of the motor would vary as the weight, and this can be assumed to be about true in the construction of artificial motors, where no two are made precisely similar, where the internal friction of small sizes is great, and where we find no such great variation in the sizes experimented with as to attract our attention to this law.

Orthogonal Flight.

Two kinds of dynamic flight have been mentioned—*i.e.*, beating flight and sailing flight. We may now alter these terms a little to "orthogonal flight" and "gliding flight." Orthogonal is a term devised and employed by the French, and indicates that kind of flight in which the weight is sustained by a direct downward dynamic thrust upon the air. Thus the insect or humming-bird, when holding its body motionless in the air, is practising orthogonal flight ; if a screw were arranged upon a vertical shaft, so that by rotating it

raised a weight through the air, that would be orthogonal flight; any arrangement of valvular wings, which are intended to open when the wing is raised and to close when the wing is depressed, and so lift an apparatus up through the air, would be designed for orthogonal flight. In an apparatus intended to operate by orthogonal flight, the action of the moving machinery is expected to produce an upward reaction of sufficient force to raise the machine, and horizontal translation is not an essential condition, as it is in gliding flight.

Perhaps an estimate of the amount of power required to raise a man vertically through the air by this means will not be uninteresting. This power must be applied so as to drive the man upward and the air downward, and this being the case, the least power will presumably be used when the greatest possible quantity of air is operated upon.

This latter statement may be illustrated by the case of a cannon and its ball. Suppose a cannon weighing 1,000 lbs. and a cannon-ball weighing 1 lb. to be free in space; the charge is exploded, and the same force—namely, the pressure exerted by the powder—acts on both the cannon and upon its ball for the same length of time while the ball is travelling from the breech to the muzzle. Now although the force and the time during which it is exerted are the same in the case of the cannon and of the ball, the power which is imparted to each, and which is conserved in their momentums, differs. By Newton's second law of motion, the velocities of the cannon and of the ball under these circumstances will be to each other inversely as their masses; the ball will therefore be traveling 1,000 times as fast as the cannon. Now since the power contained in the momentum of any mass is proportional to the square of the velocity and to the mass itself, we see that the power conserved in the cannon will be represented by $1{,}000 \times 1^2 = 1{,}000$, and that the power conserved in the momentum of the cannon-ball will be $1 \times 1{,}000^2 = 1{,}000{,}000$. The latter amount of power is 1,000 times the former. When two bodies are acted upon by the same force for the same length of time, the power conserved in the momentum of each will vary inversely as their masses.

In orthogonal flight, therefore, the greater the amount of air acted upon and driven downward, the less will be the power required. The amount of air acted upon will increase directly as the surface and as the speed employed.

All this may be shown by a numerical example. Suppose an apparatus to weigh 300 lbs., and to be supported by an arrangement, often proposed by inventors, of valved wings, which will permit the air to pass through when the wing is raised, but which will close when the wing is depressed, creating resistance and so raising the machine. Let there be two separate surfaces, one of which is being raised while the other is being depressed. In this way one will be in action at all times. Suppose that the area of each of these surfaces be 300 sq. ft., and that the resistance when they are raised through the air is nothing.

The resistance upon a plane surface in passing through the air is, as ascertained by Professor S. P. Langley, and here expressed in terms of feet and seconds instead of in the metric units he gives:

$P = 0.00152\ V^2 S.$

P = pressure in pounds.

V = velocity in feet per second.

S = area in square feet.

By the assumption made $P = 300$ lbs. and $S = 300$ sq. ft. (for only 300 of the 600 sq. ft. are acting); substituting in the equation, we have

$$300 = 0.00152\ V^2\ 300;$$

from this we find that $V = 25.6$ ft. per second, and since a horse power is equal to 550 foot-pounds per second, the power required will be

$$\frac{300 \times 25.6}{550} = 14 \text{ H.P.}$$

This is plainly more than a man could develop. If the man weighed 150 lbs. there would be remaining $(300-150) = 150$ lbs., which would be the possible weight of the material out of which to construct 600 sq. ft. of surface.

Suppose that we make the entirely unreasonable assumption that two such wings, each with 1,200 sq. ft. of surface, could be constructed out of 150 lbs. weight, we should then have the following:

$$300 = 0.00152\ V^2\ 1{,}200 \qquad V = 12.8 \text{ ft. per second,}$$

and

$$\frac{300 \times 12.8}{550} = 7 \text{ H.P.}$$

We thus see that with four times the surface only one-half the horse power is required to accomplish the same work—that is to say, the power required will vary inversely as the square root of the surface, or

$$\frac{1}{14} : \frac{1}{7} : : \sqrt{300} : \sqrt{1{,}200}.$$

This may be shown mathematically, for neglecting constant coefficients the formulæ just used become

$$P = S\ V^2 \text{ and horse power} = P\ V.$$

If we double the surface and retain P constant, we have

$$P = 2\ S \frac{V^2}{2} = 2\ S\left(\frac{V}{1.414}\right)^2.$$

The new surface then becomes $2\ S$, and the new velocity

$$\frac{V}{1.414},$$

so that the new horse power is

$$P\left(\frac{V}{1.414}\right).$$

That is, the surface has been doubled and the power required has been decreased by the square root of two.

We thus see that the power required will vary inversely as the square root of the surface.

We may undertake to ascertain by these equations how large a surface would be required for a man to raise 300 lbs. vertically by his muscles with the particular arrangement supposed. Suppose that a man can develop continuously one-eighth of a horse power, we can make the proportion :

$$\frac{1}{14} : \frac{1}{0.125} :: \sqrt{300} : \sqrt{x} \quad x = 3,760,000 \text{ sq. ft.} = 86.5 \text{ acres};$$

but in order to enable the man to fly we must have another surface similar to this, because one will be inactive all the time, making 173.0 acres of surface to be constructed out of 150 lbs.

While it is evident that man cannot by his own muscular efforts expect to overcome gravity in this way, an artificial motor may perhaps do so. If we can construct 600 sq. ft. of surface of the weight of 150 lbs., and a motor developing 14 H. P. and weighing only 150 lbs., two requirements which it is probably not impossible to meet, it is seen that the apparatus would rise, but it would have to go without an attendant.

We infer from this that the use of valvular wings, which has been so often proposed, is not advantageous, and that orthogonal flight is scarcely practicable.

The use of the screw in this connection will be discussed later.

Gliding Flight.

It being seen by calculation and by general observation that orthogonal flight requires much more power per pound weight for its accomplishment than gliding flight, and since the lack of power is one of the greatest obstacles in the way of successful aerial navigation for man, it is almost certain that of the two kinds of flight the latter will be the one adopted. It is more economical in the matter of obtaining support and also of obtaining horizontal translation, without which flight is worthless.

The force which gives support in gliding flight is easily realized. Take a fan, let the flat side be almost horizontal, and move it horizontally through the air; if the forward edge be raised a trifle higher than the rear edge, a strong uplift will be felt. Now there will be some resistance to horizontal motion, but if the inclination of the fan be very small, this resistance will also be small. It will be very much less than the upward thrust. This is the principle by virtue of which birds obtain support in gliding through the air. The large bird glides upon apparently horizontal wings, but which are, in fact, slightly inclined, and the force which sustains him is the same as that which tends to raise the fan. At the angles at which it is estimated that birds usually sail, the

horizontal resistance will be from one-sixth to one-thirtieth that of the upward pressure—that is to say, the force required to move the bird's wing horizontally through the air, and so sustain his weight, is only one-sixth to one-thirtieth of the weight itself. It must be remembered in this connection, however, that force is not power. The above statement simply means that 1 lb. of pressure applied horizontally to a thin inclined plane, moving through the air at the angle and the speed usually adopted by sailing birds, will keep that plane in motion horizontally, and will sustain from 6 to 30 lbs. weight.

This pressure upon the air by the supporting surface is, by the well-known laws of fluid pressure, always normal to the surface. The direction of this pressure is at right angles to the surface at all times, and this being the case, the resistance to horizontal motion and the uplift can be determined by simple trigonometrical resolution of forces, if the normal pressure be first ascertained.

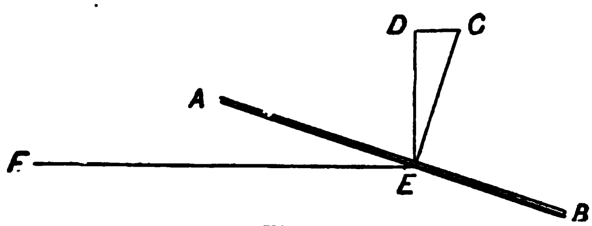

Fig. 1.

If $A\ B$, fig. 1, be the surface upon which the horizontal current $F\,E$ is impinging, the direction of the force created by the pressure of that current will be along the line $E\ C$, which is always at right angles to the plane; and if the length of the line $E\ C$ represents the amount of this normal pressure, the length of the line $E\,D$ will represent the uplift, and the length of the line $D\ C$ will represent the resistance to horizontal motion. It is seen that the line $E\,D$ is equal to $E\,C$ multiplied by the *cosine* of the angle $D\ E\ C$, and that the line $D\,C$ is equal to the line $E\,C$ multiplied by the *sine* of the same angle. This angle, $D\ E\ C$, is equal to the angle $F\,E\,A$, which is the angle of inclination of the plane. The uplift, therefore, is equal to the normal pressure multiplied by the *cosine* of the angle of inclination, and the resistance to horizontal motion is equal to the normal pressure multiplied by the *sine* of the angle of inclination. It is also seen that the resistance to horizontal motion is equal to the uplift multiplied by the tangent of the angle of inclination, or that the uplift is equal to the resistance divided by that tangent. We have therefore:

$$W = N \cos. @ \quad R = N \sin. @ \quad R = W \tan. @ \quad W = \frac{R}{\tan. @},$$

in which W is the weight or the uplift, R is the resistance to horizontal motion, N is the normal pressure upon the plane, and @ is the angle of inclination.

Now since power is resistance multiplied by velocity,

$$H\,P = V\,R = V\,W\,tan.\,@,$$

in which V is the velocity and $H\,P$ the power.

If $H\,P$ is in English horse power, it will be 33,000 foot pounds per minute, 550 foot-pounds per second, or 375 mile-pounds per hour; if we make W the weight in pounds and V the velocity in miles per hour, we shall have:

$$H\,P = \frac{V\,W\,tan.\,@}{375},$$

or

$$H\,P = 0.002667\ V\,W\,tan.\,@,$$

in which $H\,P$ is the English horse power, V is the velocity in miles per hour, and W is the weight or uplift in pounds. The author believes this last formula to be original, and it is seen that it is applicable to any fluid.

It will be noted that the extent of the surface is not an element in the formula. If, therefore, we can ascertain the velocity, the weight, and the angle employed by sailing birds, we can ascertain the power consumed by them in mere support. Two of these elements are comparatively easy to estimate, the weight and the velocity; but the angle of inclination is still an unknown factor, and the difficulties of measuring that angle are so great, that it is doubtful if any reliable data will be obtained in regard to this matter for some time to come.

There is another element which enters into gliding flight, which has not been considered, and this is the supporting surface. It is important, both in view of its size and its shape. If it is small, a high velocity or a considerable angle will be required to sustain the weight, and this, as we see by the formula just given, means great power; if it is too large, its own weight will be considerable, which, we also see by the formula, will require more power, and even if the area were determined most accurately, its shape would be a matter of serious consideration. As will be seen further on, the power consumed will vary not only when the shape of the surface is varied from plane to curved, but also, in the case of surfaces with unsymmetrical outlines, as different edges of the surface are placed at the front or at the side.

THE SUPPORTING SURFACE—ITS EXTENT.

The knowledge of the extent of the supporting surface required for gliding flight will be quite as necessary to know as the amount of power, and our ability to navigate the air by means of sailing flight will depend quite as much upon the accurate determination of one as of the other. The actual sur-

face will depend upon the efficient pressure which can be obtained from the air per unit of area or the pressure per square foot. Regarding this pressure there is still some doubt; but a considerable number of experiments have been tried, and their reliability is such that it is probably safe to trust the conclusions drawn from them. The most reliable of these experiments are also the most concordant. The air is such a light, subtle and transparent fluid that its movements are only determined with the greatest difficulty, and the causes which may interfere with those motions are so slight as to be indiscernible. Whirlpools and eddies are so easily formed that there is little wonder that the experiments which have been tried exhibit wide discrepancies, and that no two of them exactly agree.

In determining the extent of the supporting surface necessary to sustain any weight, the first thing to be ascertained is the weight which 1 sq. ft. will sustain when exposed to a current of air of a known velocity *and at a known angle*. As has been shown, the weight which such a surface will support is a function of the normal pressure, so that if we can ascertain the normal pressure and the angle we shall easily calculate the weight and also the resistance to horizontal motion.

Now, the pressure of the current of air upon a surface exposed at right angles to it has been the subject of very many experiments, but the proper coefficients are still in doubt.*

When, however, the plane is inclined to the current, this normal pressure decreases. The ratio between the normal pressure when a plane is at right angles to the current, and when that same plane is at some other angle than a right angle, is not absolutely known, but it is known with considerable exactness. The rational formulæ which have been proposed at different times, although by such men as Rankine and Weisbach, seem to be in entire disagreement with the facts and also with each other, and the only formulæ which appear to have value are the empirical ones. In making this statement the author must except M. Ch. de Louvrié, whose formula, proposed in the *Revue de l'Aeronautique* for 1890, 4me *livraison*, seems to have more merit than any other rational formula which has been suggested.

At the instance of Mr. Chanute, and from data furnished by him, the writer constructed in 1891 a chart upon which he plotted down graphically the results of various experiments which have been made upon planes, inclined at various angles to the horizon and exposed to currents of air or driven against

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* Engineers generally use Smeaton's formula, which is known to give excessive results, but to be "safe." Converting the metric units given by Professor S. P. Langley, on page 22 of his "Experiments in Aerodynamics," into English units, we have, for the normal pressure upon a plane, when that plane is at right angles to the current:

Langley, $P = 0.00327\ V^2 S$,
Smeaton, $P = 0.005\ V^2 S$.

In which P = lbs. pressure per square foot,
V = velocity in miles per hour,
S = area exposed in square feet.

the air. These comprised the experiments of Vince, Thibault, Hutton, De Louvrié, Skye (in New Zealand), the British Aeronautical Society, and W. H. Dines, and were found to show wide discrepancies at some of the angles; yet by eliminating various probable sources of error, it was concluded then that the empirical formula proposed in 1828 by Colonel Duchemin, of the French army, was the most satisfactory. This seems more nearly to fit the experiments than any other which has been proposed; and although an empirical formula, and therefore not exactly accurate, it is doubtful whether the most exhaustive experiments will improve it greatly. This formula is:

$$P_r = \frac{2\ sin.\ @}{1 + sin.^2\ @} \times P,$$

in which Pr is the normal pressure when the plane is inclined, P is the normal pressure when the plane is at right angles to the current, and @ is the angle of inclination. The formula simply gives the percentage:

$$P_r = \not{s}\ P, \text{ and } \not{s} = \frac{2\ sin.\ @}{1 + sin.^2\ @}.$$

The conclusion thus reached was confirmed a few months later by the publication of Professor Langley's experiments, which he showed to agree closely with the formula of Colonel Duchemin, and it was deemed that the table of percentages of pressures at various angles which had been constructed might be used without risk of falling into serious error.

Subsequently the attention of the writer was called to the rational formula of M. Ch. de Louvrié, which has already been alluded to. This is based upon considerations that the resistance to the escape of the films of air impinging upon a flat plane must be in proportion to the amount of obliquity of that plane, and led him to the formula:

$$N = P\left(\frac{2\ sin.\ @\ (1 + cos.\ @)}{1 + cos.\ @ + sin.\ @}\right),$$

in which

N represents the normal pressure on an inclined plane;
P " pressure on a plane at right angles;
@ " angle of incidence.

This formula takes no account of the rarefaction upon the back of the plane which is known by experiment to take place in consequence of the action of the escaping films of air, and yet it agrees more closely than that of Duchemin with the experiments of Hutton and of Dines, which show that a maximum of pressure occurs when the plane is inclined at an angle of incidence of about 55°.

Once that we have a formula expressing the relation between the normal pressure, when the plane is exposed at any angle

to the current, with the pressure upon the same plane when it is at right angles to the current, we can easily calculate the components of that normal pressure in a vertical and in a horizontal direction—the "lift" and the "drift"—by simply multiplying the normal by the sine and by the cosine of the angle of incidence.

In this way the following table was calculated for Mr. O. Chanute, who published the results of Duchemin's formula in the RAILROAD AND ENGINEERING JOURNAL for October, 1891.

It will be noted from the table that the maximum normal pressure shown by the De Louvrié formula occurs at about 70°, while it occurs at 90° by the Duchemin formula. Also that the maximum per cent. of the lift occurs at 39° by the De Louvrié and at 36° by the Duchemin formula. A general observation shows that there must be such a point of maximum lift, for there is certainly no lift when the plane is perfectly horizontal and the current strikes only its edge, nor is there any uplift when the plane is at right angles to the current; there is some uplift at all intermediate angles, and so there is some angle where the uplift will be the greatest. Now, if this were the angle at which flight is to be maintained, we should then be enabled to use the least possible surface for support, which means the least possible weight in the supporting surfaces; but it is noticed that the resistance to forward motion at 36° is quite large, and this means that considerable power must be employed to drive the apparatus through the air to obtain the support.

The two formulæ agree closely at the small angles (from 2° to 10°) at which birds usually sail, and for such angles a formula, which is perhaps quite as accurate as either, is simply $N = P\,2\,sin.\ @$, or still more simply, $N = P\,0.03\ @$, with @ expressed in degrees—that is, at 10° inclination the normal pressure, in proportion of the pressure upon the same plane when exposed to the same current and at right angles to it, is:

$$10 \times 0.03 = 0.30, \text{ or } 30\%.$$

These last formulæ will, of course, not apply to large angles; they are not general formulæ as the Duchemin formula, for at an angle of 90° the Duchemin formula will give unity as a result. They are, however, fairly correct up to an angle of 20°, perhaps as correct as the Duchemin formula itself, and gliding flight, if accomplished, will, in all probability, be practised at lower angles than 20°.

Upon the whole, however, it will be best to base computations upon the Duchemin formula as being the one most generally accepted.

Now that we have established, approximately at least, the ratio of supporting power and of resistance—of lift and of drift—which will obtain at various angles of incidence, we are prepared to inquire what should be the extent of the supporting surface to carry a given number of pounds in the air.

NORMAL PRESSURES, LIFT AND DRIFT, IN PERCENTAGES OF RECTANGULAR PRESSURE, BY FORMULAS OF CH. DE LOUVRIÉ, AND OF COL. DUCHEMIN.

DE LOUVRIÉ. $N = P \frac{2 \sin. \alpha (1 + \cos. \alpha)}{1 + \cos. \alpha + \sin. \alpha}$.				DUCHEMIN. $N = P \frac{2 \sin. \alpha}{1 + \sin.^2 \alpha}$.			
Angle.	Normal.	Lift.	Drift.	Normal.	Lift.	Drift.	Angle.
1	0.0347	0.0347	0.000608	0.035	0.035	0.000611	1
1½	0.0531	0.0531	0 00181	0.052	0.052	0.00186	1½
2	0.0688	0.0688	0.0023	0.070	0.070	0.00244	2
3	0.1021	0.1020	0.0053	0 104	0.104	0 00543	3
4	0.1345	0.1344	0.0094	0.139	0.139	0.0097	4
5	0.1670	0.1640	0 0142	0.174	0.173	0.0152	5
6	0.1995	0.1962	0.020	0.207	0.206	0.0217	6
7	0.2100	0.2283	0.028	0.240	0.238	0.0293	7
8	0.260	0 2573	0 036	0.273	0.270	0.0381	8
9	0.290	0 2863	0.045	0.305	0.300	0.0477	9
10	0.319	0.314	0.055	0.337	0 332	0.0585	10
11	0.348	0.342	0.066	0.369	0.362	0.0702	11
12	0 376	0.368	0.078	0.398	0.390	0.0828	12
13	0.404	0.394	0.0909	0.431	0.419	0 0971	13
14	0.431	0.418	0.104	0.457	0.443	0.1155	14
15	0.457	0.441	0.118	0.486	0.468	0.124	15
16	0.483	0.464	0.133	0.512	0.492	0.141	16
17	0.508	0.486	0.149	0.538	0.515	0.157	17
18	0.533	0.507	0.165	0.565	0.538	0.172	18
19	0.558	0.528	0.181	0.589	0.556	0.192	19
20	0.582	0.547	0.198	0.613	0.575	0.210	20
21	0.606	0.566	0.216	0.637	0.594	0.228	21
22	0.630	0.584	0.236	0.657	0.608	0.246	22
23	0.652	0.599	0.255	0.678	0.623	0.264	23
24	0.673	0.615	0.274	0.700	0.639	0.286	24
25	0.693	0.628	0.294	0.718	0.650	0.304	25
26	0.712	0.640	0.313	0.737	0.662	0.323	26
27	0.732	0.652	0.333	0.752	0.670	0 342	27
28	0.751	0.662	0.354	0.771	0.681	0 362	28
29	0.770	0.672	0.374	0.786	0.686	0.382	29
30	0 789	0.682	0.395	0.800	0.693	0.400	30
31	0.807	0.691	0.416	0.815	0.698	0.421	31
32	0.825	0.700	0.437	0.828	0.702	0.439	32
33	0.842	0.706	0.459	0 843	0.706	0.459	33
34	0.858	0.711	0.481	0.853	0.707	0.478	34
35	0.874	0.715	0.502	0.867	0.706	0.498	35
36	0.889	0.718	0.523	0.878	0.709	0.516	36
37	0.903	0.722	0.545	0.885	0.709	0.532	37
38	0.919	0.724	0.567	0.894	0.705	0.551	38
39	0 934	0.726	0.589	0.902	0.701	0.569	39
40	0.948	0.726	0.609	0 910	0.697	0.586	40
41	0.959	0.724	0.630	0.918	0.693	0.602	41
42	0.970	0.721	0.650	0.926	0.688	0.619	42
43	0.981	0.715	0 670	0.934	0.683	0.638	43
44	0.992	0.711	0.691	0.941	0.676	0.654	44
45	1.000	0.707	0.707	0.945	0.666	0.666	45
50	1.046	0.672	0.800	0.966	0.621	0.740	50
60	1.098	0.549	0.951	0.990	0 495	0.857	60
70	1.105	0.378	1.039	0.997	0.341	0.937	70
80	1.071	0.186	1.055	1.000	0.174	0.985	80
90	1.000	0.000	1.000	1.000	0.000	1.000	90

The five important elements which enter into the problem of support in gliding flight are :

1. The weight of the apparatus.
2. The area of supporting surface.
3. The velocity of forward motion.
4. The angle of inclination.
5. The horse power required.

These are all interdependent, so that their relations to each other are best shown by a diagram.

The chart herewith* given, designed by the writer,† shows

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* The object of the accompanying chart is to show some of the elements which enter into soaring flight (viz., area, velocity, angle of inclination and power required) and their relations to each other. The diagram is constructed by the use of three formulas, two of which are empirical, the other rational.

The first is that for wind pressure upon a plane surface at right angles to it ; it is :

$$P = 0.00327\ V^2\ S, \qquad (1)$$

(P) being the pressure in lbs., (V) the velocity of the current in miles per hour, (S) the area of the exposed surface in square feet. The coefficient 0.00327 is that determined by Professor S. P. Langley, and may be found upon page 22 of his "Experiments in Aerodynamics," the figures there being .0.008, which is the coefficient to be employed if metric units are used

The other empirical formula is :

$$Pr = P\frac{2\ sin.\ A}{1 + sin.^2\ A}. \qquad (2)$$

(P) being the same quantity as above, (A) the angle between the plane surface and the current upon it, (Pr) is the resulting pressure, and according to the law of fluid pressure is normal to the surface. This formula is by Colonel Duchemin, of the French Army, and has been in existence a number of years.

These are probably the most accurate formulas of their kind in existence. In any case their inaccuracies will not affect a number of conclusions which may be deduced from the diagram.

The rational formula is :

$$HP. = 0.00266\ W\ V\ tan.\ A,$$

in which $HP.$ is English horse-power, and (W) the weight in pounds supported by the aeroplane.

If (Pr) is the normal pressure upon a plane surface the lateral pressure will be $Pr\ cos.\ a$. The lateral pressure is that at right angles to the current, and in the case of the aeroplane is the uplift, W:

$$W = Pr\ cos.\ A. \qquad (3)$$

The pressure upon the plane in the direction of the current (R) is :

$$R = Pr\ sin.\ A.$$

By definition, power is resistance into velocity. The horse-power is 33,000 ft. lbs. per minute, or 375 mile lbs. per hour, since V and R are in the latter units :

$$HP. = \frac{V\,R}{375} = 0.00266\ V\ R.$$

Substituting for R its value as above :

$$HP. = 0.00266\ V\ Pr\ sin.\ A. \qquad (4)$$

Dividing equation (4) by (3) we obtain :

$$HP. = 0.00266\ W\ V\ tan.\ A.$$

† See p. 30.

the proportions in which these elements enter into gliding flight when the weight is made the ruling unit and the other elements are given in ratios of 1 lb. of weight.

From the following diagram it may be noted by the reader that no particular extent of supporting surface (of area in square feet per pound of weight) can be said to be absolutely the proper one to employ unless the available horse power be first known. In other words, that the sustaining surfaces of aeroplanes will have to be proportioned to the power by which they are to be driven, just as in the case of gliding birds, where the wing and body surfaces vary from 3.62 sq. ft. to the pound in the case of the swallow, to 0.68 sq. ft. to the pound in the case of the tawny vulture.

Thus, if an apparatus with its load is to weigh 2,000 lbs., and has a sustaining surface of 2,000 sq. ft. in extent, or at the rate of 1 lb. to the square foot, we see from the chart that, when gliding horizontally at an angle of 8°, it will require a speed of 34 miles per hour, and 0.0125 × 2000 = 25 H.P. to obtain support; while if the angle be reduced to 4°, it will need a speed of 47 miles per hour, and 0.0087 × 2000 = 17.4 H.P. to be supported.

If the weight be the same, and the area be 1,000 sq. ft., or at the rate of 0.5 sq. ft. per pound, then at an angle of 8° the speed must be 48 miles per hour, and 0.0176 × 2000 = 35.2 H.P. must be exerted, while if the angle become 4°, then it

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If the value of P as given in (1) is substituted in (2):

$$Pr = 0.00327\, V^2\, S \frac{2 \sin. A}{1 + \sin.^2 A}.$$

From (3)

$$Pr = \frac{W}{\cos. A}.$$

Substituting, we obtain:

$$W = 0.00327\, V^2\, S \frac{2 \sin. A \cos. A}{1 + \sin.^2 A}.$$

If (W) is made equal to one pound, or unity:

$$V^2 S = \frac{1 + \sin.^2 A}{0.00327 \times 2 \sin. A \cos. A}.$$

It is seen from this that when (W) is constant, that ($V^2 S$) is a constant for each value of (A), and it was by the use of this expression that the heavy curved lines in the chart were plotted. If in the formula:

$$HP. = 0.00266\, W\, V \tan. A,$$

(W) is made equal to one pound, or unity, then:

$$V \tan. A = \frac{HP.}{0.00266}.$$

It is seen from this that when (W) is constant, that ($V \tan. A$) is a constant for each value of $HP.$, and it was by the use of this expression that the lighter curved lines were plotted over the heavier ones. The author is under obligations to Mr. O. Chanute for the information which enabled him to construct the chart.

will need a speed of 67 miles per hour, and 0.0125 × 2000 = 25 H.P. to sustain the 2,000 lbs.

It must be remembered, however, that the horse power indicated on the chart is merely that required to sustain the weight through the action of the plane, and takes no account of the further power required to overcome the head resistance due to the car containing the machinery and the passengers, and to the edges of the framing and spars which impart the necessary stiffness. The power shown on the chart is that needed for

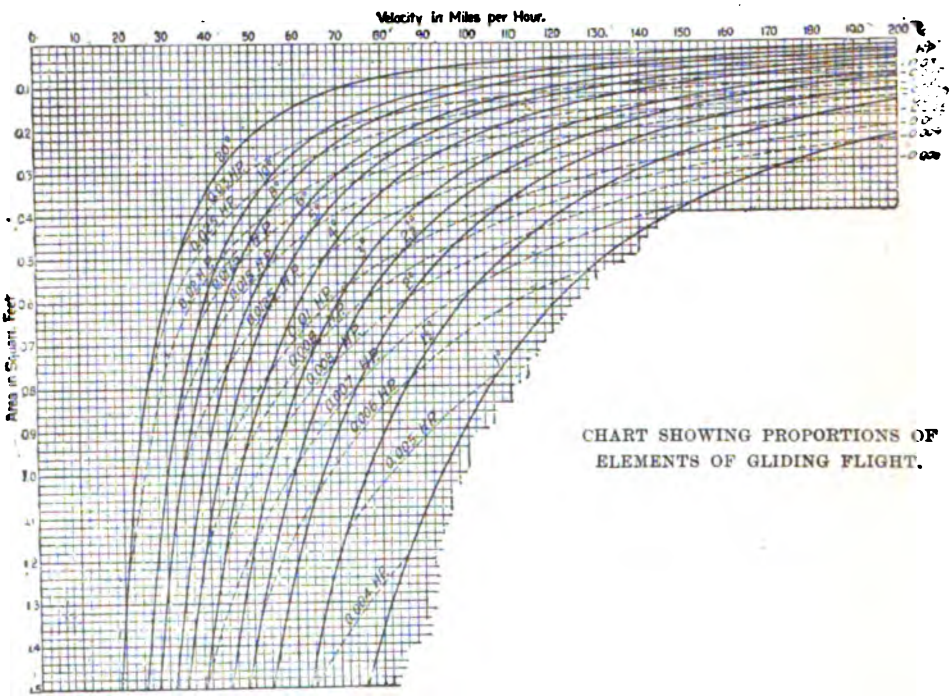


CHART SHOWING PROPORTIONS OF ELEMENTS OF GLIDING FLIGHT.

support only, and the other elements of resistance requiring power to overcome them will be treated of further on.

It is not claimed, therefore, that this chart is accurate, but only that it is as accurate as knowledge which is public at the present time enables it to be made. It may not be true that 100 lbs. will require 100 times as much surface, or 100 times as much horse power to support it in sailing flight as 1 lb. will. As a matter of fact, it will require proportionally less; but the relations between large-sized planes and small-sized

planes are not yet mathematically established, and so cannot be shown upon the chart. Notwithstanding these imperfections it is probable that the chart has been figured for safe data—in other words, that no more horse power or surface will be required for supporting heavy weights than is indicated by the chart, so that a fair idea of the proportions of the different elements can be obtained. Even admitting these defects to be fatal to the use of the chart for economical constructional purposes, several deductions may be drawn from it quite as well as if it were absolutely accurate.

Suppose that the area of the supporting surface remains constant and that the speed increases, it will be seen that the angle of inclination and the horse power consumed will decrease. This will be seen by following one of the horizontal lines from left to right and noticing how the curved lines are cut by it.

If the angle of inclination remains the same and the speed be increased, then the required area of the supporting surface will be decreased and the power required increased. This can be seen by following one of the curved lines indicating the angle from the left to the right.

If the power remain constant and the speed be increased, then both the area and the angle will be decreased.

It will be seen that there is a point of maximum curvature in the lines which shows the angles of inclination required, and that these points, in the cases shown, fall between 80 and 100 miles per hour. Now, the use of the smallest possible angle is desirable, both for speed and power, but there will be, of course, some practical limit to this decrease in the size of the angle which can be used ; this limit can probably only be determined by practice. Admitting, however, that there is such a limit to this decrease in the angle of incidence, we see that if the speed is much below the point indicated by the point of maximum curvature the supporting surface must be greatly increased in size, and this will, of course, mean additional weight and probably weakness. Thus, if the smallest angle which can be safely used be 5°, we see that if the speed be decreased from 70 miles per hour (which is the speed indicated by the point of maximum curvature) to 40 miles per hour, that the area of supporting surface required will be increased from 0.36 sq. ft. per pound to 1.10, or more than three times ; while the power required will decrease only from 0.015 to 0.009, or considerably less than one half. In considering this matter it must also be remembered that the weight of the supporting surface will probably increase much faster than the extent of that surface, while the weight of the motive power will vary about in the direct ratio. All of this shows how important it is to use a high speed, so far as these considerations go. On the other hand, we see from the chart that when the angle remains the same and the speed is increased the area is diminished ; it is, in fact, diminished as the square of the speed, and that the horse power is increased directly as the velocity. The higher the speed, therefore, the

less the supporting surfaces will weigh, and the more the motor. Development in this direction, then, must be stopped at some point, for we cannot make the supporting surfaces indefinitely small nor the motor indefinitely large.

The conclusions which we draw are that there is an economical speed for gliding flight—that it is more than 30 and probably less than 200 miles per hour when only the questions of the power and surface required for support are considered. Further on we will show that this 200 miles per hour will be considerably reduced by other considerations.

If we assume, as we must, that the pressure upon the supporting surface is normal to that surface, then the power required for support is truly indicated by the chart; but power will be required for other purposes than support. The exposed surfaces of the apparatus, which do not aid in support, all meet with resistance in passing through the air, and this resistance will be entirely independent of the extent of the supporting surface, depending upon the design of the machine. This resistance and the power required to overcome it must be made the subject of separate considerations and calculations. Speaking mathematically, they can only be connected with the resistance and power met and required for support by plus or minus signs.

THE SUPPORTING SURFACE—ITS OUTLINE.

By outline of the supporting surface will be meant the shape of the bounding lines, whether square, rectangular, curved, etc., and also the "aspect," which is a term used by Professor Langley to indicate the position of the sides of the surface with reference to the direction of the current. It will make a great difference in the resulting effect whether the long side of a rectangular aeroplane be placed in the direction in which the current moves, or whether the short side be so placed. The effect will be the greatest when the short side is in the direction of motion.

Suppose that we have such a surface that the particles of air which strike the front edge find their way of escape at the rear and not at the sides; as has been seen, those particles will create their greatest pressure upon the front edge and their least upon the rear portions of the aeroplane. If now we increase the length of the front edge and decrease the width of the aeroplane, but keep the area the same, it is seen that the most effective surface has been increased and the least effective surface decreased.

In the aeroplane which is gliding at a very small angle we have increased the mass of air upon which it operates by just as much as we have increased the length of the front edge. Although not quite true, we can almost say that in the case of small angles the mass of air upon which an aeroplane acts will be the velocity multiplied by the length of the forward edge of the aeroplane. This, it is seen, is merely the superficial area passed over by the aeroplane in a unit of time. It is the spread of the aeroplane multiplied by the distance which it

travels, and, as has been said, will nearly correspond to the mass of air acted upon when the angle of flight is small.

Now, we have seen that the horse power required to obtain support from a given area of surface will vary as the cube of the speed, and therefore, if it be the spread that varies, then the velocity will vary inversely as the cube root of the spread, or, to state this mathematically :

$$V = \frac{1}{\sqrt[3]{spread}}$$

That is to say, if we compare two planes, of equal area but of different spread, supporting the same weight, and both soaring horizontally at the same angle of inclination, the velocity required to maintain this flight will vary inversely as the cube root of the spread of the planes.

Thus, if we have a plane 30 × 4.8 in., soaring at an angle of 5°, with the 30-in. side transverse to the line of motion and at a speed of 50 miles an hour, it supports 2.2 lbs. in horizontal flight, then a plane carrying the same weight with the same area, but measuring 6 × 24 in., and moving with the 6-in. side transverse to the current, and placed at the same angle of inclination will require to soar a speed of :

$$V = 50\sqrt[3]{\frac{30}{6}} = 85.5 \text{ miles per hour for support.}$$

Thus, we see that there is a material advantage in making an aeroplane oblong, and driving it with the spread in the line of motion ; but in practice the extent of this advantage will be modified by the increased resistance due to the longer framing or spar at the forward edge, and by the increased weight of the framing required to resist the strains due to the increased length of leverage. The extent of this modification will depend upon the design of the aeroplane.

THE SUPPORTING SURFACE—CHARACTER, SHAPE.

The character of the supporting surface needs less consideration in the present state of knowledge than any other of the features connected with that surface. The fact that the friction of the air upon a smooth surface is so small as to be immeasurable except by the most delicate instruments indicates that but little advantage is to be expected in this direction. A number of promoters of aerial navigation have believed that there was some virtue in the peculiar surface of the feather ; that in some way or other the feather caught the moving air, and that the result of the forces arising was such that the flying body received a forward impulse. There are some reasons for thinking that this may not be entirely untrue, or at least that those who so believe have not been overcredulous, although none of their theories can as yet be mathematically verified. If there is any truth in the theory that a surface of feathers will be beneficial, it will be ascertained by experi-

ments which will be made after aerial navigation is a fact. The most that can be said at this time is that the smoother the surface the better the results, and that if that surface be as smooth as it is possible to make it, the friction of the air upon it and the power required to overcome it will be negligable.

But the shape of the supporting surface is a subject which must eventually receive considerable attention, although perhaps not a very great deal in the initial apparatus. It is possible that in the development of the bird's wing very nearly the proper shape has been reached. In the discussion of the action of the air upon any surface upon which it impinges, it is evident that if we could ascertain just what change of motion was produced in the particles of air affected, we could probably estimate the forces required to create those motions, and so obtain the forces which act upon the surface and their points of application. The determination of the change of motion in the particles of air which are affected by the opposition of the plane to their motion is very difficult to ascertain, and mere guesses as to how the stream lines flow are not of much value; but we can assume that some are deflected from their course more than others, and so require more force than others for their change of motion.

If, for instance, the plane is exposed at right angles to the current, it is probable that the particles of air assume courses something as shown in fig. 2 in passing around the plane:

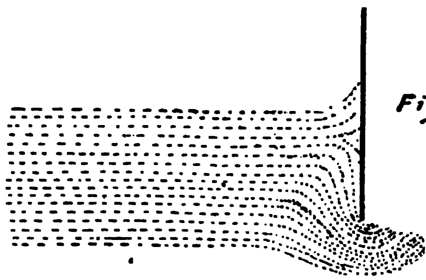


Fig. 2.

Now, if the force required to deflect these particles from their direct course could be ascertained, and also the change of velocity which they undergo, together with the same elements concerning the particles of air at the back of the plane, the force acting upon the plane might be ascertained; but this would be a very complicated operation, both experimentally and mathematically, and the rational settlement of the question in that way is hardly to be thought of at this time. The theory will, however, enable us to account for some known facts, and to state some unknown ones which are of importance.

If, in fig. 3, a particle of air strikes the plane A B at the point 1, it is seen that that particle is at once deflected into a

course parallel to *A B*, so that all the force acting on that particle of air is applied at the forward edge of the plane. The reaction is, of course, upon the plane itself. The particles of air which are lower down possibly do not reach the plane at all, but are deflected by the upper particles ; at any rate, they do not suffer such a violent change of motion, do not require the same force for their change of direction, and so, conse-

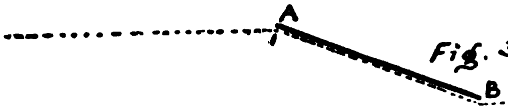


Fig. 3.

quently, do not exert the pressure upon the plane that the upper particles of air do. The result is that the center of pressure on an inclined plane is forward of the center of area. This is shown by experiment, but the exact ratio of this change has not been satisfactorily determined.

This variation of the position of the center of pressure in an inclined plane from the position of the center of figure has been the subject of some experiment in the case of plane surfaces, and a formula for its variation has been proposed, although that formula will probably only apply to square planes. In the case of the latter it has been ascertained that the center of pressure may vary from the center of the surface by as much as 30% of the length of the side of the plane. The formula, as proposed by Mr. Joëssel, is

$$D = (0.2 + 0.3 \, sin. \, @) \, L,$$

in which D is the distance from the front edge of the plane, @ is the angle of inclination, and L is the length of the side of the square plane. This formula is the best that we have at the present, and although it would hardly do to use it in construction of oblong aeroplanes if much depended upon its accuracy, it will give the reader a fair idea of the position of the center of pressure in an inclined plane exposed to a current of air, but the result may be quite different if the plane is not square.

Suppose the plane to be a foot long in the direction *A B*, and only an inch broad. It is seen that a particle of air, on striking this surface, will not pass along the line *A B* and thus travel 12 in., but that it will take the route of least resistance, and that is to one side or the other, in which case it will only have to travel a half inch or less. This being the case, the lower particles of air, which in the former case were only slightly deflected and did not reach the plane, will now strike the plane, and will receive as great a change of motion as the upper particles. This being the case, we must expect that the plane, which is long in the direction of the current and short transversely to that current, will have its center of pressure more nearly the center of the surface than a plane which is square, like the one previously discussed.

If the surface, instead of being a plane, is a curved surface, then a different condition is introduced.

It is seen, from fig. 4, that the particle of air which meets the curved surface at the point 1 is deflected somewhat, and that when it reaches the point 2 it is deflected still more, and more at 3, and so on. The force which the particle receives from the surface is more evenly distributed than in the first case, where a plane instead of a curved surface was used. Now, in comparing the two surfaces, plane and curved, it is seen that in the latter case we may expect the center of pressure to be nearer the center of surface than in the former, and, furthermore, that the latter is a more efficient surface than the former. In the case of the plane surface the rear does not receive the same pressure as the front, while in the curved surface each portion can be made to do an equal amount of work, for this curve will be a parabola. It is seen, therefore, that such a curve can be formed for each angle of inclination that the center of pressure shall be nearly at the center of the surface. If the curve is very sharp, the angle will more nearly

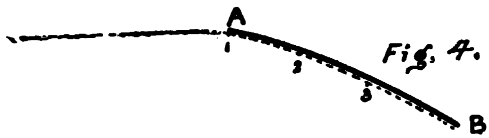


Fig. 4.

approach the horizontal; if the surface be plane, then the center of pressure will coincide with the center of the surface only when the angle of inclination is 90° from the current. If a surface such as is shown in fig. 4 be swung on pivots and placed in a current of wind and permitted to move freely, it will take a position such that the center of pressure and the center of the surface will coincide. If it be turned to some position such as is shown in fig. 4 there will also be found a point where the center of pressure and that of the surface coincide; but in this case the surface must be held there, for its equilibrium is unstable, and it will turn if it can.

We can see now why the surface of a bird's wing is curved and not flat. It is a more efficient surface when curved, and less of it is required to the work demanded than if the surface were flat. The curvature of the bird's wing, however, varies considerably, and is not the same in full flight as when simply spread out and not in action. The most effective shape, therefore, will have to be ascertained by experiment, as well as the motion of the center of pressure on surfaces of various shapes and aspect—an important line of research, upon which, unfortunately, we at present know very little.

THE STABILITY.

Granting that an apparatus may be built which will possess the necessary supporting surface and a sufficient motor and

motive instrument to propel it through the air at satisfactory speed, yet aerial navigation might be far from being accomplished. The machine might still lack stability; it might topple over with the slightest gust of wind or with the least change in the position of the center of gravity caused by the movements of the operator or of the cargo.

On the land and on the water stability is easily secured; but in so intangible and yielding a fluid as air, the case is very different. Here the stability must be provided for in all directions—transversely, and fore and aft—and while the bird apparently does this with ease, this is due to the fact that he is aided by the intuition of life. An artificial machine to be capable of performing in useful time the various manœuvres by which the bird preserves his equilibrium, should, if possible, be automatic. Hints as to how this may eventually be accomplished can best be given by considering separately the transverse and the longitudinal stability.

TRANSVERSE STABILITY.

The transverse stability will probably not be difficult to obtain automatically by simply imitating the birds. If we observe the soaring turkey buzzard we note that much of the time the wings are inclined upward and outward at a diedral angle from the body, as in fig. 5.

Fig. 5.

The effect is that, if either side tends to rotate and tip downward it receives increased pressure from the air, while the other side receives diminished pressure, and so the equilibrium is re-established.

Moreover, the bird's body acts as a keel, and the air resists the tendency to rotate sideways around the center of gravity. Both of these devices will be available for artificial machines by setting the supporting surfaces at a diedral angle to each other, and by adding longitudinal keel cloths, which should preferably be placed below the center of gravity. Such keel cloths may terminate in a vertical rudder, and thus allow of steering the machine, but they will need special devices for bringing them into a horizontal position when alighting on the ground, as otherwise they might be injured.

LONGITUDINAL STABILITY.

Longitudinal stability will probably be more difficult to attain than transverse stability, and there are fewer experiments upon which to base mathematical calculations which will be reliable, than is the case with transverse stability.

It is evident enough that it may be secured on the same principle in both cases. A fixed plane at the stern of the apparatus, or both at the bow and stern, which shall be inclined at quite a different angle to the horizon than the angle taken by the main supporting plane, will probably give great additional longitudinal stability, but it will also be somewhat wasteful of power, and the principal problem in aerial navigation is how to economize power. Other methods, therefore, should be sought for to make the machine stable fore and aft.

It has been proposed to drive an aeroplane lengthwise, and it would seem at first consideration that the longer a plane be made in the direction of motion the more stable it would be in a longitudinal direction, but this may not be the case. It is probable that if the plane be very long in the direction of motion and narrow transversely to that direction, then the particles of air which strike the forward edge do not escape at the rear of the plane, but find their way to the sides and pass away there. This action would, of course, result in other particles of air striking the plane at other points than the front edge, and also passing away at the sides. The effect of such motion would be that the center of pressure would be nearer the center of the long side of the plane, and that its absolute change in position would be greatest.

Now, this movement of the center of pressure is a corrective one, because when the plane tips either forward or back the center of pressure either advances or retreats, and so creates a turning moment which tends to tip the plane back into its original position. It will be seen, therefore, that the center of gravity of the apparatus must be made to coincide with the position which the center of pressure will assume at the angle at which the apparatus is to sail.

This is the method, with or without the addition of a fixed plane at the stern at a different angle, which has been employed to confer longitudinal stability upon such experimental models as have been tried hitherto, but it is not entirely satisfactory, and the writer knows of sundry experiments which he is not at liberty to make public, that seem to indicate that better methods may be employed.

THE ACTUAL POWER.

Up to this point we have only been considering the power required to obtain support from the air; but a very considerable amount of power will be required for another and very different purpose, and that is to overcome the resistance of the air upon other than the supporting surfaces. These surfaces will be fixed in their area and shapes, and in these respects will offer a uniform resistance, which from the knowledge we now have should vary as the square of the velocity.

From the formula $HP. = 0.00266\ W\ V\ tan.\ @$ it is seen that it is not necessary to know the extent of the supporting surface to ascertain the power required for support, but, however, in order to determine the power required to overcome

the resistance upon other than the supporting surfaces, the extent and the shape of these surfaces must be known.

For the want of a better term, we will call the resistance upon other than the supporting surfaces the head resistance.

In order to determine this head resistance, it is first necessary to determine the extent of the area exposed, and then the modifying coefficient corresponding to the shape of that area. This can only be done by designing a machine, measuring the area of the closed portions of the vertical transverse projection, and then applying the proper coefficients for flat, round, or tapering surfaces, as may be the case. From such data the resistance and the power required to overcome it may be calculated.

As it is not the object of this paper to indicate any especial form of apparatus, a hypothetical case will be assumed.

Suppose that a machine be constructed of 1,000 lbs. weight, and that its projection forward shows an area of 20 sq. ft. outside of the supporting surfaces. Suppose that curved edges can be used such that the resistance created by their passing through the air will be one-fifth of that due to a vertical plane surface of equal sectional area. That is, that if their projection is 20 sq. ft., as has been assumed, the resistance arising from the body passing through the air will be the same as if the surface were $20 \div 5 = 4$ sq. ft. in a single plane and at right angles to the current.

The resistance will then be from the formula:

$$P = \frac{0.00327 \times 20 \times V^2}{5}.$$

Since there are 375 mile-pounds per hour in a horse power, the horse power required to overcome the head resistance will be:

$$(HP.)_1 = \frac{0.00327 \times 20 \times V^3}{375 \times 5} = 0.0000349\ V^3.$$

We will now assume a supporting surface of 1 sq. ft. per pound, or 1,000 sq. ft., and try to ascertain the power required for horizontal flight in terms of the velocity. To obtain this by the use of the Duchemin formula would introduce trigonometrical functions that would make subsequent mathematical calculations very complex. The work can be very much simplified and but little accuracy sacrificed, as will follow.

Substitute for the Duchemin formula, which is:

$$\frac{2\ sin.\ @}{1 + sin.^2\ @},$$

the expression 0.03 @, with @ expressed in degrees, and for *tan.* @, which will be used in the calculations, use 0.0176 @,

with @ expressed in degrees. These expressions are accurate enough for the small angles used in flight. The cosine of @ will be considered as remaining at unity.

These changes being made, we have, by the use of the formula with Langley's coefficient:

$$1{,}000 \text{ lbs.} = 1{,}000 \text{ sq. ft.} \times V^2 \times 0.00327 \times 0.03\,@ \times \cos.\,@ \text{ (or 1).}$$

From the above equation we find that:

$$@ = \frac{10{,}200}{V^2}.$$

We have seen that the power required for support is:

$$(HP.)_2 = W \times V \times \tan.\,@ \times 0.00266,$$

and substituting for W and *tan.* @ their values, we have:

$$(HP.)_2 = 1{,}000 \times V \times 0.0176\,@ \times 0.00266\,;$$

then substituting the value of @ just found, we have:

$$(HP.)_2 = 1{,}000 \times V \times \frac{0.0176 \times 10{,}200}{V^2} \times 0.00266 = \frac{477.5}{V}$$

The total ($HP.$) consumed is then:

$$(HP.)_1 + (HP.)_2 = 0.0000349 \times V^3 + \frac{477.5}{V}, \text{ or}$$

$$HP. = \frac{0.0000349 \times V^4 + 477.5}{V}.$$

Applying calculus, and solving for a minimum of $HP.$, we have $V = 46\ 5$, and this is the velocity in miles per hour for a minimum of work. It is seen that there is such a point of minimum work, for, as has already been observed, if the area remains constant the power required for support will diminish as the velocity increases, while the head resistance will increase when the velocity increases. It is also seen from the different parts of the expression that the head resistance, $(HP.)_1$, increases as the cube of the velocity, while the power for supports, $(HP.)_2$, decreases as the velocity increases.

Substituting the value of V just found, we see that

$$(HP.)_1 = 0.0000349\ (46.5)^3 = 3.50,$$

or power required for head resistance;

$$(HP.)_2 = \frac{477.5}{46.5} = 10.25,$$

or power required for support.

Total $HP.$, 13.75,

and the angle of flight will be

$$\frac{10.200}{(46.5)^2} = 4^\circ\ 43'.$$

These figures of 1 sq. ft. per pound, 46.5 miles per hour, 4° 43′ angle, and 0.010 *HP.* per pound for support, agree well with those which the chart shows, indicating that the mathematical modifications have introduced no great error.

If the utmost power that can be exerted upon the machine, all losses due to slip, friction, etc., being deducted, is, say, 25 *HP.*, we can write:

$$25 = \frac{0.0000349\ V^4 + 477.5}{V};$$

and solving for V, we find that the utmost speed of which this apparatus is capable will be 81.9 miles per hour, and from the same methods just used we find that the angle will be 1° 12′. It is doubtful if any such small angle could be continuously and certainly maintained in practice, so that it would seem as if the apparatus proposed would have to be modified in some way if the speed of 81.9 miles per hour is to be attained. In this case, in which the *HP.* used is 25 and the speed 81.9 miles per hour, the power required to overcome the head resistance will be:

$$HP._1 = 0.0000349\ (81.9)^3 = 19.2$$

and the power required for support will be:

$$HP._2 = \frac{477.5}{81.9} = \underline{5.8}$$

$$\text{Total } HP., \quad 25.0$$

By a comparison of the speed and power consumed in support at 46.5 miles per hour with the speed and the power consumed in support at 81.9 miles per hour, it is seen that such power varies inversely as the speed, or that:

$$46.5 : 81.9 :: 5.8 \cdot 10.25.$$

We may say roughly, and quite truly for small angles, that if the velocity remains constant, we may double the angle, halve the supporting surface, and double the power required for support.

Referring to our aerial craft, which is travelling at a speed of 81.9 miles per hour at an angle of 1° 12′ and consuming 5.8 *HP.* in support, we see that if the angle should be increased to 3° 36′ (an angle which might be safely held), the surface will be reduced from 1,000 sq. ft. to (1,000 ÷ 3) 333 sq. ft., and the power required for support increased from 5.8 to (5.8 × 3) 17.4 *HP.*, thus making the total power required at the speed of 81.9 miles per hour (19.2 + 17.4) 36.6 *HP.* instead of 25 *HP.*, as was required when the angle which could be used was 1° 12′.

All these things go to show that the question of proportions in the construction is an involved and intricate one. If we are to use small horse powers, we will need to have large surfaces, which feature means, in its turn, large weights; and if we use small surfaces we must have large powers, which, in its turn again, means large weights. There is one thing that is plain from all these calculations, and that is that the smallest angle of inclination at which the aeroplane can be held will be the best as regards economical and rapid flight. Could this minimum be ascertained, the determination of the other proportions would be much simplified; it is, however, doubtful if this desirable knowledge can be obtained, even approximately, until the first apparatus shall have made its successful flight, and until that time those attempting construction would probably do well to base their calculations on some angle which must be large enough, and probably too large.

It is seen, from the way in which the resistance upon other than the supporting surfaces (hull, spars, etc.) increases when the speed increases, how necessary it is that those surfaces should receive the most careful attention as regards their shape, for the resistance met by such surfaces will be modified very much by this proper forming, just as the resistance of the marine craft is greatly changed by a suitably shaped hull. The swift aerial craft will have to be compact, and all projecting points which are liable to catch the air will have to be carefully avoided. The fish or bird shape has been adopted for marine craft, and as it seems to offer less resistance than any other, it will probably be adopted in aerial vessels. One large item of resistance in marine craft which can probably be neglected in the design of aerial vessels is the friction of the surrounding fluid upon the sides of the vessel. It can be almost positively stated that the longer an aerial vessel is in proportion to the area of its greatest cross-section, the less will be the resistance which it will meet in passing through the air.

THE MOTIVE INSTRUMENT.

The number of instruments by which the machine can be moved through the air are not many, and the choice should not be hard to make. They are, so far as the writer is aware, (1) the reaction or jet system; (2) the feathering paddle; (3) the "wave" motion, the wing, or oscillating fin, and (4) the screw.

1. The advocates of the first system propose to drive the vessel through the air, or water, as the case may be, by forcing some fluid in the opposite direction, and using the unbalanced pressure for the motive force. Let us suppose that steam is to be used in direct reaction.

It is seen that if there be 100 lbs. of steam pressure in the boiler and a 1-in. aperture at the rear, there will be a pressure of just 100 lbs. which is unbalanced and a force of just 100 lbs. tending to drive the steam which must escape through the aperture in one direction and to propel the boiler in the

other. The amount of fuel and of water which will be required to maintain this pressure of 100 lbs. will be measured by the amount of steam which will pass through an aperture of 1 sq. in. section at this pressure of 100 lbs., and with no opposing resistance save that of the atmospheric pressure. This is at about the rate of 1,380 ft. per second, and the amount of water required each second will be 2.35 lbs., or 8,460 lbs. per hour. This means that at the rate of evaporation of 25 lbs. of water per H.P. per hour, and the burning of 1 lb. of oil for each 25 lbs. of water, that such a device will require a boiler with a capacity of 338.4 H.P., and that the supplies required will be 8,460 lbs. of water and 338.4 lbs. of oil per hour, a total of about 8,800 lbs. per hour for the purpose of securing a thrust of 100 lbs. This will be seen to be quite impracticable.

2. The feathering paddle has been proposed many times as an aerial propeller and also as a marine one, but it has never been adopted successfully. Its action is easier to comprehend than any of the others, and it was referred to in the first part of this paper under "Orthogonal Flight." The principle is to move a paddle so that its surface will be at right angles to the direction of motion of the craft during the working part of its (the paddle's) movement; then turn it parallel to the said direction during the return movement. In the first motion it meets with all the resistance that its surface is capable of, and in its return the only resistance encountered is that upon the edge. The objection to this form of propeller is comparative, for it might be made to answer if there were nothing better. It is seen, however, that the surface is in action only one-half of the time, and that when it is on its return to the point of beginning of the effective stroke it is quite idle. To be sure, when it is in action it is as effective as it is possible to make it, for the force applied by its means upon the air is precisely in the opposite direction to the motion that it is desired to impart to the apparatus. This advantage, however, is more than offset by the total inactivity of the surface during half of the time, so that, as compared with the screw, the actual surface of the feathering paddles is quite inefficient. The operation of the feathering paddles will also require some moving parts which cannot add to its ease of operation and may be quite a detriment. The machinery required to turn the paddles at each revolution which they make is the great objection to the feathering paddle in marine practice, where its trial has not resulted in marked success. The greatest reason of all, however, for the use of a screw in preference to the feathering paddle lies in the fact that the former will create a greater thrust for the application of the same power, and this is the case even if it be assumed that the feathering blade consumes no power at all in its action of feathering or in its return to the beginning of the next stroke.

3. The wave motion and the wing or oscillating fin can be considered together here. The three figures below show the principle employed by each.

The flexible fin, fig. 7, has been applied unsuccessfully to marine craft, and the greatest obstacle to its success has been the oscillating motion required in its operation. As the fin swings to and fro it bends under the action of the opposing force and strikes the water at an angle, so that the resulting force will tend to drive the vessel ahead. All such propellers have been commercial failures, whether they secured the feathering motion by the elasticity of the materials of which the fin was composed, or whether some special machinery was employed for that purpose. Even if such elasticity or machinery did all that could be asked of it, still the vibrating fin could not under any circumstances be a more efficient propeller than the screw.

Fig. 6.

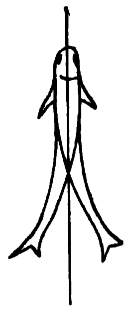


Fig. 7.

The wave motion propeller, fig. 6, is a step in advance of the vibrating fin, in that it is probable that in its employment some of the sudden shocks due to the reciprocating motion of the fin would be at least in part destroyed. The "wave" motion propeller is composed of some fabric only one side of which is firmly secured; this side is raised up and depressed with the result that the fabric is thrown into a series of waves which, driving the air backward, drive the apparatus forward. It is a little difficult to see just how more than one wave is of service, for the thrust from following waves must be transmitted through the fabric, and of such transmission the fabric would hardly be capable. Reduced to one wave, this motive instrument becomes the fin.

The action of the wing is pretty well understood. The two drawings, fig. 8, show it first in rising and then in descending. In the first the inclination of the wing is such that a forward thrust results; in the latter that inclination is reversed so that the forward thrust again results. It is seen that it is necessary for the wing to come to a complete stop at the end of each half stroke, and to change its angle of inclination. The latter is done continuously throughout the stroke, so that when the extremities of the half stroke are reached the plane of the wing is about horizontal.

Now, suppose that when the wing is descending it does not change its angle of inclination, but keeps it constant; suppose that it does not stop when it reaches the bottom of the half stroke, but continues on around underneath the body upon the other side and then down on the side from which it started, it is seen that we have a one-bladed screw; and if we can imagine the other wing turning in the same manner but in the opposite direction, so as to balance the turning moments, we shall have an instrument which is quite as effective for forward thrust as the best natural wing, and much more so than any artificial wing that could be constructed.

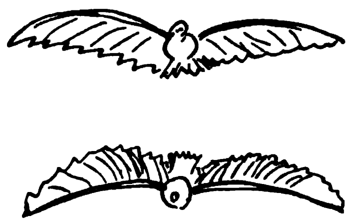


Fig. 8.

4. The great advantage in the use of the screw is the fact that its surface is always active; there are no *dead* points; that its motion is continuous and not reciprocating; that its angle of inclination can remain constant throughout the whole revolution, and that it requires no special machinery or moving parts in its construction and operation; it can be made in one piece. In the use of the screw, man has departed from the practices of nature, as he has in nearly all other mechanical development. The argument that man must imitate the birds if he desires to fly seems to the writer to be rather weak. In the development of marine and land vehicles man certainly has not followed the method of propulsion used by the quadruped, the biped, or the fish, and yet he has succeeded in surpassing all these in size, speed, and endurance, and this in a single vehicle in which all these qualities are combined.

THE SCREW PROPELLER.

The adoption of the screw propeller as an instrument of propulsion in transportation work is comparatively recent.

Its use in steam vessels is now almost universal, but its use as an instrument of propulsion for aerial craft has been very limited. It has been applied to four balloons, only one of which can be called navigable, as it was the only one which succeeded in returning to its starting-point.

Unfortunately the experimental data regarding the action of aerial screws is very meager; but there are some principles which will apply and some results which may be predicted quite as well as if they were based upon experiments made upon existing screws; moreover, there is no doubt that the action of the aerial screw can be better determined theoretically than the action of the marine screw. No marine engineer can predict what the action of a new form of marine screw will be, and, as with respect to the shape of the hull, each maker has his "doctrine." Where the best results are re-

quired, the practice now is to try several forms of screws on the same vessel and to adopt the best. The reason for this uncertainty lies in the fact that the marine screw is frequently only partially immersed; that the pressure upon the lower blades of the screw is greater than upon the upper, and that consequently the water will flow away from the upper blades more easily than from the lower; that the friction of the water is a considerable quantity, and will vary greatly with the material of the screw and with its finish, and that the action is greatly influenced by the form of the hull behind which the screw operates. It is seen that all or nearly all of these conditions are quite indeterminate, and even if they were determined, the mathematical determination of the result would be a work of such complexity as to render it next to impracticable.

In the case of the aerial propeller many of these indeterminate conditions disappear. The screw is always completely immersed, the pressure upon the lower and upon the upper parts of the screw are the same, and the air can flow away from the screw with equal facility in all directions. It may be that the aerial screw will be affected by the shape of the aerial vessel which it drives, but it is probable that such screw will be in the center of the end projection of the vessel—that is, the vessel will be symmetrical around the screw, and that the fluid will reach it from all sides with equal ease. This is not the case with the marine propeller, and the consequence is that complications in its action continually appear which cannot be forcseen, and which frequently cannot be accounted for.

We may assume that like any body moving through a perfect fluid, the pressure upon the blade of the screw will vary as the square of the speed; and if this be the case the efficiency of the screw will vary inversely as the speed. By efficiency is meant the number of pounds thrust per *HP.* of energy developed, and it can be obtained by dividing the total thrust by the total *HP.* required to produce it. If E represents the efficiency or the pounds thrust per *HP.*, and T represents the total thrust,

$$E = T \div HP.$$

Now, if the velocity of the screw be doubled, the actual thrust or the total pounds of thrust will be increased by four: but the *HP.* consumed will be increased by eight, so that the efficiency will be decreased by two, or inversely as the velocity.

Further discussion of this feature is almost unnecessary, for it depends upon the well-known principle that the power required to move any body through a fluid increases as the cube of the velocity, and that the resistance met by the body increases as the square of the velocity.

Since the thrust is immediately dependent upon the resistance, and will vary with it, neglecting all constants, we will have

$$T = V^2 \quad HP. = V^3$$

$$E = \frac{T}{HP.} = \frac{V^2}{V^3} = \frac{1}{V},$$

or efficiency varies inversely as the velocity.

The truth of this is shown in the experiments made upon the screw of the French war balloon, in which the power, thrust, speed, etc., were accurately measured, and the results agree with the foregoing theory almost exactly. The experiments were published, and this theory announced in the *Revue de L'Aeronautique* in the number for 1889, page 97.

We may also assume that the thrust of the screw will increase directly with the area, and representing the area by S, and neglecting all constant coefficients,

$$T = S\,V^2, \text{ and } HP. = S\,V^3.$$

Suppose that the surface be doubled, but that the velocity be so changed that the thrust will remain the same, then we will have

$$T = 2\,S\,\frac{V^2}{2}.$$

The new velocity will then be $\sqrt{\frac{V^2}{2}}$, and the power required will be $2\,S\left(\sqrt{\frac{V^2}{2}}\right)^3$. Reducing this expression, we have new $HP. = \frac{S\,V^3}{1.414}$, while the old $HP. = S\,V^3$. We see that by doubling the surface and reducing the velocity so that the thrust will remain the same, we have decreased the $HP.$ by $\frac{1}{1.414}$, or by the square root of ½.

In the first instance the efficiency was:

$$\frac{S\,V^2}{S\,V^3}, \text{ or, } E = \frac{1}{V}.$$

In the second instance the efficiency was:

$$\frac{S\,V^2}{\frac{S\,V^3}{1.414}} = \frac{1.414}{V}.$$

Comparing this with the first efficiency, we see that the efficiency has increased by the square root of the factor by which the area has increased. The efficiency, therefore, of an aerial propeller will vary directly as the square root of its

area. Since the diameter varies as the square root of the area, we may say that the efficiency of two similarly shaped screws will be directly proportional to their diameters.

The fact that the efficiency of an aerial propeller will vary as the square root of its area is also shown in the discussion of orthogonal flight

The pitch of a propeller screw is the distance in the direction of the axis that would be traversed by the screw during one complete revolution in an unyielding medium. The rate of pitch of the blades of the screw is one of the elements in its construction which must be carefully determined.

The pressure of the air upon the blades of the screw is of course normal to the surface, and this being the case, it is seen from the discussion of the action of the pressure of the air upon an inclined plane that the thrust will be the normal pressure multiplied by the cosine of the angle, while the resistance to turning will be the normal pressure multiplied by the sine of the angle. Now, if the velocity of the turning screw remains constant, it may be neglected in comparing different horse powers under other varying conditions for turning the screw, and we shall have merely, by neglecting all constant coefficients :

$$HP. = N \sin. @,$$

N being the normal pressure and @ being the angle of pitch.

In the same way the thrust will be :

$$T = N \cos. @,$$

and the efficiency will be :

$$E = \frac{N \cos. @}{N \sin. @} = \cot. @.$$

Other things remaining the same, the efficiency of a screw will vary as the cotangent of the angle of pitch. Since for small angles the cotangent is inversely proportional to the angle itself, we may say that if the angle of pitch be small, the efficiency of the screw will vary inversely as that angle.

In the case where the angle is small and is increased and the velocity kept constant, the thrust is of course increased ; but the resistance is increased in far greater proportion. If we consider the screw as an inclined plane, and apply the Duchemin formula to it, we see that at small angles the normal pressure and the thrust are both increased just about as the angle is increased, but that the resistance to turning will be increased by the normal pressure multiplied by the sine of the angle, and the latter at small angles increases just as the angle. If, therefore, we double the angle, keeping the velocity constant, we shall double the normal pressure and double the thrust, but we shall increase the resistance four times, thus making the efficiency only one-half of what it was before. At small angles, therefore, the efficiency will vary inversely as the angle of pitch.

In this connection it may be well to discuss the angle of maximum thrust, for it is plain that there is such an angle. There is no thrust when the angle of pitch is at zero degrees, nor is there any thrust when the angle is 90°, but there is some thrust at all intermediate angles. The velocity of the screw remaining the same, the thrust will increase as the angle of pitch is increased from zero degrees, and it will also increase as the angle of pitch is decreased from 90°. There must, therefore, be an angle of maximum thrust. In the case of the inclined plane we found that this angle was by the Duchemin formula 35¼°. The only experiment upon screws which is in possession of the writer, and which indicates the angle of maximum thrust, shows that it is at about 35°, thus confirming the Duchemin formula.

It is plain, of course, that the efficient screw must have an angle of pitch which is less than that for the maximum thrust, for if the angle is increased beyond that of maximum thrust, the screw will not only be inefficient by reason of the great angle, but it will be peculiarly so, for the resistance to rotation or the power consumed will be increased and the thrust will be decreased. At angles which are less than the angle of maximum thrust, any increase in the angle of pitch results in an increase of resistance and of power consumed, but it also results in an increased, not a diminished thrust.

If the weight of the prime mover, the engine, were *not to be considered*, then the screw for aerial propulsion should be as small, turn with as high velocity as possible, and be set at the angle of maximum thrust, for all these features would aid in obtaining the greatest amount of thrust out of a screw of the least size and consequent weight. If the efficiency is not to be considered, then it can be thus constructed. But the efficiency is a most important feature; it is as important as the weight of the prime mover itself, and so we see that for the most efficient screw we should have one which is as large as possible, turns with the lowest speed possible, and use the smallest possible angle of pitch.

All these conditions are incompatible with the construction of a light screw, for they mean in fact that the most efficient screw will tend to be the heaviest.

It is seen, therefore, that the design of the screw will be a matter in which considerable judgment based upon reliable experiments must be used; and the most that can be definitely stated at the present time is that the screw should be made as large, turn with as small velocity, and be constructed with as small an angle of pitch as other conditions will permit. We may at this time consider one of these conditions to be that the screw shall remain of uniform size, and it may be well to discuss the question of the pitch and the speed which shall be given to a screw of a constant area. It will be difficult—probably impossible—to make the size of the screw adjustable; the diameter must always remain the same. There will then be two possible ways of altering the thrust—one by changing the speed and the other by changing the pitch.

We have seen that the power will vary as the cube of the speed, and that the thrust will vary as the square of the speed; and we have also seen that the thrust will vary as the normal pressure upon the blades multiplied by the cosine of the angle of pitch. If the angle is small, this cosine will remain practically at unity, and the normal pressure will vary as the angle itself, so that, neglecting all constant coefficients, we have:*

$$T = V^2 P,$$

P being the angle of pitch of the blades.

The resistance to turning, however, will be proportional to the normal pressure multiplied by the sine of the angle of pitch; for small angles these will vary about as the angles themselves, so that:

$$HP. = V^3 P^2.$$

The efficiency will then be:

$$\frac{T}{HP.} = E = \frac{V^2 P}{V^3 P^2} = \frac{1}{V P}.$$

Now, to obtain a given thrust we have seen that the velocity may be low and the angle of pitch great, or the velocity may be great and the angle of pitch low; but if the thrust remains the same, we shall have T equal to some constant, say 1, and we shall have:

$$1 = V^2 P, \text{ or } V = \frac{1}{\sqrt{P}}, \text{ and } P = \frac{1}{V^2}.$$

Substituting these expressions in the value of E, we have:

$$E = \frac{1}{\frac{1}{\sqrt{P}} P} = \frac{1}{\sqrt{P}},$$

and

$$E = \frac{1}{V \frac{1}{V^2}} = V.$$

Under these circumstances the efficiency will vary directly as the velocity and inversely as the square root of the angle of pitch. In other words, if we are to obtain a given thrust from a screw of fixed size it must be turned with the highest velocity practicable and at the smallest angle of pitch, if the greatest efficiency is to be obtained.

Applying, then, the principles that have been discussed in the chapters on the Supporting Surface, we see that the blades of the screw should be narrow in the direction of motion, that they should be of parabolic shape, and that the amount of pitch should be the same for all portions of the blade. These fea-

* The author's meaning in what follows would be more clear if the sign of variation were used instead of the sign of equality.—ED.

tures have been tried and adopted in marine screws with success, though it must be said that the varying conditions of such service prevent rigid adherence to these rules.

The parabolic shape of the blades is known as the increasing pitch in the direction of the length of the screw, for the pitch increases from the front edge of the blades to the rear, so that a particle of water which strikes the blade at the forward edge receives a constantly accelerated motion, until it escapes at the rear of the blade. This, as has been seen in the discussion on inclined planes, would not be the case if the blade were a plane surface, for the particle would receive all or very nearly all of its motion at the forward edge of the blade. The total amount of pitch should be the same, for if it is not then there will be some portions of the blade which will drag through the water instead of affording useful thrust, and there will be other portions which will be doing more work than they ought, with the result that they will put water in motion which might better be left quiet. This uniform total pitch for all parts of the screw is the decreasing rate of pitch from the center toward the outside. If the total pitch of the propeller be 5 ft. and the radius be 4 ft., and the angle of pitch at the circumference be 6°, then the angle of pitch at a point 2 ft. from the center, or half way between that point and the circumference, should be about 12°, in order that the total pitch may be the same for each part.

It is seen that if this principle is carried out the angle of pitch at the center of the screw will be 90°, and that it will gradually decrease to the outer extremity. We have seen that there must be an angle of maximum thrust, and that probably that angle is at 35° or very close to it. That portion of the surface of the screw which is set at a greater angle than 35° is therefore peculiarly inefficient, and if the exposure of this portion of the surface of the screw could be avoided, the efficiency would be increased. It is not impossible that this may be done in the case of the aerial propeller, that such portion of the screw as is set at an angle in excess of the angle of maximum pitch may be entirely shielded from the air; and this arrangement, too, would give opportunity and room for strengthening the screw at its center, where the greatest strains exist.

The slip of the screw is a matter which has received great attention from the makers of marine screws, and must also receive attention from the maker of aerial screws; but it is probable that the erratic character which so often puzzles the marine engineer may be modified in the case of the aerial propeller, where the conditions may be said to be more uniform.

Slip may be defined as that distance which ought to be traversed, but which is lost through the imperfections of the propelling instrument; or it may be considered as the wasted power—power which should have been used in putting the craft in motion, but which has been consumed otherwise.

When the total pitch of the screw is given the slip will be the difference in the distance between that pitch and the dis-

tance that the vessel actually travels while the screw is making one revolution. If the pitch of the screw is 5 ft. and the vessel only travels 4 ft. while the screw is making one turn, then the slip is 1 ft. per turn.

In the other case the slip is expressed as a proportion of the power lost. From the thrust of the screw and the speed of the vessel the *HP*. consumed by the vessel may be ascertained. Now the power applied to the screw can also be measured by suitable instruments, and it will be found to be considerably in excess of the power determined in the former case. The difference will be the slip expressed in *HP*. lost. Some of this power is lost in friction and unavoidable losses which will occur in all machines; but most of it is lost through the inefficiency of the instrument of propulsion, the screw.

This power is lost in the surrounding medium upon which the instrument of propulsion must work; and when this is understood several important conclusions may be drawn. If the medium in which the screw works were unyielding, then there would be no slip, no more than there is when the locomotive wheel works upon the dry rail; all of the power in that case would pass into the craft, and it would all appear as useful; if the surrounding medium yields some, then a part of the power passes into it; some motion is communicated to it, and the power so used is lost for purposes of propulsion. If the medium yields so perfectly that the craft remains stationary, as is the case when a steamboat is moored to the dock and her engines put in motion, then all of the power is used in slip—that is, in putting the fluid in motion—and none of it is used in propulsion.

It is seen, then, that it is very important to so design the instrument of propulsion that it will create as little motion as possible in the surrounding fluid.

Now the function of the screw is to create thrust. To do this it operates upon a fluid medium, which will of course move under the influence of the force applied. The force or thrust which the screw creates will be in proportion to the mass of fluid moved, and also to the velocity at which it is put in motion. The power consumed in putting this mass of fluid in motion, and which, as has been seen, is lost, is proportional to the mass put in motion and to the *square* of the velocity at which it moves. From this it is seen that, in order to obtain a given thrust with the least loss of power, the mass of fluid acted upon should be as great as possible, and the velocity imparted to it as little as possible.

We may compare the screw with the reaction device first mentioned under discussion of the motive instrument, and see that the thrust, whatever it may be, reacts upon the fluid and puts it in motion. If a screw 1 in. in diameter can be imagined to revolve with such velocity that the thrust amounts to 100 lbs., we shall then have quite as inefficient an apparatus as the reaction jet discussed. The efficiency of the screw arises from the fact that it puts a large quantity of the surrounding fluid in motion, but at a very slow velocity; it is, in fact, just as

much of a reaction engine as the jet system discussed, but by taking advantage of the fact that the thrust developed will vary as the velocity imparted to the fluid, while the power required will vary as the square of that velocity, the efficiency appears by maintaining that velocity at as low a point as possible.

The action of the screw may also be compared with that of the fan blower, which it has been proposed to use in aerial navigation with the object of creating thrust. The general plan is to create a current of air by means of the fan, and then by directing this current or jet in any desired direction to obtain a reaction in the opposite direction. The general design of the fan blower is such that it will move a small quantity of air at a high velocity, which, as we have seen, is just what is not desired. The screw is, in fact, a fan blower so modified that it will put a large quantity of air in motion at a small velocity; and any modification of the fan blower with the object of causing it to work on a large quantity of air and at a low velocity will, in all probability, also cause it to resemble more and more the screw propeller.

Since all motion of the surrounding air represents so much power wasted, and it is seen that it is desirable to leave that fluid in as motionless a state as may be possible, not only should the blades of the screw be narrow, in order that the air may not be acted upon for too long a time and so put in motion, but those blades should be so separated that one blade will not disturb the air upon which the next following one must act. For this reason it has been found, both in the case of marine and of air propellers, that multiplicity of blades tends to inefficiency of action, and the consequence is that in nearly every case two blades will form the most efficient screw. For obvious reasons one blade is impracticable.

These theories, then, would seem to indicate that the most efficient screw, if both the *HP.* consumed and the weight of the motor be considered, will be made of two long and narrow concave blades, the total pitch to be uniform for all parts of the screw, the central portion of the screw so shielded that no portion of the blades, the angle of pitch of which is greater than that of maximum thrust, shall be exposed. The screw to turn with as high a velocity as practicable at all times, and the thrust to be varied by changing the angle of pitch.

The construction of an aerial screw with a variable pitch will be in all probability much less of a problem than the construction of such a device for marine service. The marine screw is generally concealed from view, is very liable to become fouled, and is quite inaccessible when in operation. The use of adjustable parts under such circumstances is to be avoided if possible; but in the case of the aerial screw most of these objections are removed. The screw will always be in sight, will not become fouled, can probably be made accessible, and a device for adjusting the pitch can doubtless be used with greater facility than in the case of marine vessels. It has been seen that the change of thrust should be effected by

a change of pitch instead of a change of velocity, the latter to be and to remain as great as the numerous other conditions which will be met with will permit.

As a motion of translation is imparted to the screw its efficiency will decrease, and the amount of this decrease can be best understood by considering the action of the paddle-wheel rather than that of the screw. Suppose the paddle-wheel of a steamer which is tied to the dock is turning so that a thrust of 40 lbs. is obtained and 1 *HP.* is employed. This thrust results from the paddles moving through still water, and the power all passes into slip. Now suppose this same steamer to be anchored in a current moving with the velocity of 3 miles per hour, and that the same total thrust of 40 lbs. is to be maintained. It is plain that the paddles must move just 3 miles per hour faster. The speed of the paddle through the water must be the same as before; but inasmuch as the water is moving 3 miles per hour faster, the actual speed of the paddle must be increased by 3 miles per hour in order that the relative speed may remain the same. Since there are 375 mile pounds per hour in each *HP.*, the additional *HP.* required will be $\frac{40 \times 3}{375} = 0.32$, and the total *HP* required to obtain the thrust will be $1 + 0.32 = 1.32$.

Suppose that the boat, when free, required a thrust of just 40 lbs. to force it through the water at the rate of 3 miles per hour, then the actual *HP.* necessary to expend upon the craft itself will be, as we have just seen, 0.32 *HP.* But suppose that we are to develop this thrust by means of a paddle-wheel which will give 40 lbs. thrust per *HP.* when working in still water. When this paddle is placed in the boat, exerts its thrust, and begins to drive the boat forward at the rate of 3 miles per hour, it is seen that the speed of the paddle must increase in order to maintain the thrust of 40 lbs. from the (relatively) moving water. If the original *HP.* be 1, we have seen that this increase of speed of the paddle will be such that the total *HP.* which it is necessary to expend upon the paddle will be the sum of that which it is necessary to expend upon the craft, and that which it is necessary to expend upon the paddle to produce the requisite thrust when the paddle is *in statu*.

The same reasoning will apply to the screw, although the analysis is not quite so evident.

The power, then, which it is necessary to apply to the screw will be that which it is necessary to apply to the craft, plus that which the screw will consume *in statu* when producing the necessary thrust.

It is seen from this how necessary it is to construct efficient screws; but it is also seen that as the speed of the craft increases the importance of efficiency diminishes, for one of the factors—that of the thrust of the screw when *in statu*—becomes a smaller proportion of the total power consumed.

Nothing has as yet indicated the actual size of the screw propeller nor the shape and velocity which will be required to

secure given results, and, unfortunately, there are so few experiments which have as yet been made public that it is quite impossible to indicate the shape and size of the screw with as great accuracy as that of the supporting surface of the aeroplane. There are, however, some things which show that the elements of the aerial screw propeller may be approximated, and that the result will not be so entirely a matter of experiment as was the case with the marine propeller in a similar stage of its development.

As has been suggested, it is by no means impossible that the action of the screw may be connected mathematically with that of the inclined plane, and there is doubtless good reason for such belief. The screw itself is, in fact, only a set of inclined planes revolving around an axis. Instead of being rectilinear, as was considered to be the case when discussing inclined plane, the motion is circular. The inclined plane, too, is nothing but the blade of a screw of which the radius is of infinite length; and it may be further stated that all of the experiments made to ascertain the normal pressure upon an inclined surface, and which have been platted, as already described, with the exception of perhaps two cases, and these the most unreliable ones, were made with a whirling engine. The plane in these cases was placed at the end of an arm perhaps 30 ft. in length; the arm was fixed at the other end and formed the radius of a horizontal circle, the plane moving in the circumference. It is seen that this apparatus was merely a screw of 60 ft. in diameter, with a small blade at the periphery, so that it is not at all unlikely that the formula derived from results with such an apparatus will apply to the screw as usually shaped, though modifications would probably have to be introduced. It seems, however, quite likely that the fundamental formulæ will be the same for the screw and for the inclined plane.

With this idea the author has attempted to analyze the only thorough and thoroughly published experiments upon an aerial propeller that he has been able to obtain.

He platted the projections of the screw of the war balloon *La France*, divided them longitudinally into six sections, and calculated the area of each section. Then, knowing the speed of each of these sections and the corresponding air pressure, and knowing also the angle of pitch of each section, it was easy to calculate by the Duchemin formula what the lift and the drift must be for each of these sections of the screw, and by summing them up to estimate the thrust exerted and the *HP.* required.

The result was that for a speed of 17 revolutions per minute the calculated thrust was 23.46 lbs., while the actual measured thrust was 17.64 lbs., or 25 per cent. less than the calculations show, and that the actual *HP.* was 0.364, or 6 per cent. more than that calculated. At the highest velocity—48 turns per minute—the measured result was 141.1 lbs. thrust, or 24 per cent. less than that calculated, while the measured *HP.* was 8.33 *HP.*, or 5 per cent. more than that calculated.

It is seen that the differences between the actual results and the calculated results are almost exactly the same in both cases, and that the application of the Duchemin formula to aerial screws, treating their surfaces as inclined planes impinging on the air, is likely to give approximately correct results.

In other experiments made by Mr. F. H. Wenham very many years ago, and with a rather crude apparatus consisting of a screw having two square blades each 7¾ in. on a side, and these being placed upon the ends of rods so that the diameter of the screw was 6 ft., the results at two velocities of 210 and 140 revolutions per minute were 84 per cent. and 15 per cent. less than the theoretical thrust which would be indicated by calculations similar to those used in the case of the French screw. Mr. Wenham's apparatus, however, generated undue friction, as may be judged from the fact that the *HP.* required was 3½ and 6 times that which would be indicated by calculations made as just indicated.

The indications both from reason and experiment are that the screw and the inclined plane can be connected mathematically, and if this be the case, the design of a screw may be undertaken with considerably more assurance than would otherwise be the case. It will be noted that in the experiments cited above the screws were each of considerable size, one of 6 ft. and the other of 23 ft. in diameter, so that the centrifugal force imparted to the air was not great. It is the writer's impression that an attempt to apply the formulæ which are used in the case of inclined planes having rectilinear motion to a screw of, say, 1 ft. diameter, and moving with high velocity, might be futile. It may also be noted in the case of the French screw that nearly one-half of the area of the screw is set at an angle of pitch of over 35°, so there is probably some reason for the statement made by Mr. Maxim in a paper in the *Century Magazine* that "of all the screws with which he had experimented, the French screw was the worst."

We have made several generalizations regarding screws, and it may be well to recapitulate them.

1. The thrust will vary as the area, as the square of the velocity, and (for small angles) as the angle of pitch.

2. The *HP.* will vary as the area, the cube of the velocity, and (for small angles) as the square of the angle of pitch.

The first statement can be easily seen to be true. If the velocity and the angle of pitch remain the same and the area be doubled, then the thrust or uplift will be doubled. We shall have substantially two planes instead of one. If the area and the angle of pitch remain the same and the velocity be doubled, then the thrust or uplift will be increased by four, for the resistance in all parts of the screw will increase as the square of the velocity. If the area and the velocity remain constant, then the thrust will vary as the angle of pitch for small angles, for we have seen that the normal pressure upon any plane will vary about as the angle itself for small angles, and that at small angles the thrust or uplift will be the normal pressure multiplied by the cosine of the angle ; but the cosine

of the small angle is very nearly unity, so that the thrust or uplift on an inclined plane will vary as the angle of pitch. We will, therefore, have:

$$T = S\,V^2\,P.$$

That the *HP.* will vary as the area is evident, for other things remaining the same, doubling the area of a plane will be the same thing as using an additional independent plane of the same size as the first, and it is plain that the *HP.* required in that case would be twice that required in the first. The *HP.* will vary as the cube of the velocity, for we have seen that the resistance on all parts of the plane will vary as the square of the velocity, so that if the velocity be doubled the resistance to turning will be increased by four; power being resistance multiplied into velocity, the power required will be increased by 4×2, or 8, which is 2^3. The power will vary as the square of the angle of pitch for small angles; we have seen that the resistance to forward motion in the case of the plane or the resistance to turning in the case of the blade of the screw propeller will vary as the normal pressure on the plane or blade multiplied by the sine of the angle of inclination; and we have also seen that the normal pressure and the sine of the angle will vary about as the angle itself for small angles, so that the velocity and area remaining the same, the resistance to rotation will vary as the square of the angle of inclination. The velocity remaining the same, the power required will vary as the resistance or as the square of the angle of inclination or pitch.

The problems regarding the efficiency may be best solved by the use of the chart which, as before said, is merely the graphical representation of results obtained by solution of two algebraic equations. The chart is constructed for a uniform uplift or thrust of 1 lb. Wherever the point of the pencil is placed upon the chart, the four elements indicated by the curves and by the figures at the top and side of the chart will be the values of those elements which will be required to sustain that pound of weight or thrust. Since the efficiency is the weight or thrust divided by the power consumed, and since the thrust as indicated by the chart remains the same—*i.e.*, unity, an increase of efficiency will be indicated by a diminution of the *HP.* and a decrease of efficiency by an increase of *HP.*; in other words, the efficiency will vary inversely as the *HP.* shown on the chart.

The efficiency will vary as the square root of the area, the angle of pitch remaining the same, and the velocity varying; or it will vary as the area, if the velocity remains constant, and the angle of pitch changes

Suppose that the angle of inclination or pitch be 4° and that the area be 1 sq. ft., then the *HP.* will be 0.0088. If we follow up the 4° line to, say, the point where the area becomes 0.5 sq. ft., we see that the *HP.* will be 0.0125; we shall then have the proportion:

0.0088 : 0.0125 :: square root 0.5 : square root 1.0,

showing that the *HP.* varies inversely as the square root of the area, or that the efficiency varies directly as the square root of the area, the angle of pitch remaining constant and the velocity varying.

But if the angle of pitch changes and the velocity remains constant, then the efficiency will vary directly as the area. If the velocity be 47 miles per hour and the area 1 sq. ft., then the *HP.* will be 0.0088; if the area be reduced to 0.5 sq. ft. and the velocity kept the same, then the *HP.* will be 0.0176, and we shall have the proportion:

$$0.0088 : 0.0176 :: 0.5 : 1.0,$$

showing that the *HP.* increases inversely as the area, or that the efficiency increases directly as the area, velocity remaining constant and angle of pitch varying.

Now, if the area and the thrust are to remain constant, there are only two conditions that can be changed, the pitch and the velocity. This being the case, the efficiency will vary directly as the velocity and inversely as the square root of the pitch. Suppose that the area be 1 sq. ft., then, at a velocity of 46 miles per hour, the *HP.* required will be 0.009, and at a velocity of 68 miles per hour the *HP.* required will be 0.006. We can make the proportion:

$$0.009 : 0.006 :: 68 : 46,$$

showing that the *HP.* varies inversely as the velocity, or that the efficiency will vary directly as the velocity.

It is seen that the angle of inclination, when making this change of velocity, has changed from 4.2° to 1.9°, and we can make the proportion:

$$0.009 : 0.006 :: \sqrt{4.2} : \sqrt{1.9},$$

showing that the *HP.* will vary directly as the square root of the angle of pitch, or that the efficiency will vary inversely as the square root of the angle of pitch.

Since, in the case assumed, the area and the thrust remains constant, if any variation is made in either the velocity or the pitch, a change must be made in the other. It is best, therefore, that this ratio should be always written in combined form—that is,

$$E^2 = \frac{V}{\sqrt{P}}, \text{ or } E = \sqrt{\frac{V}{\sqrt{P}}} = \frac{\sqrt{V}}{\sqrt[4]{P}}.$$

THE MOTOR.

The development of the motor will perhaps be the most costly part of the development of aerial navigation, and it will also be more largely a matter of empirical than of theoretical determination. It is probable that we have with fair approximation indicated in the preceding chapter how light this motor must be per *HP.*—that is to say, a minimum of 10 lbs. per *HP.*, and probably at most 15 lbs. per *HP.*, will do the work. A heavier motor than one weighing 15 lbs. for each *HP.* of energy developed will probably not answer. Now, this weight of 10 or 15 lbs. must include the motor and all that pertains to make it complete. If it be an explosive engine, the weight of

the necessary fuel for driving it and the water that will probably be necessary to keep it cool must be reckoned in with the 15 lbs., and if it be a steam engine, the weight of the boiler, the fuel and the water, as well as the engine proper, must be taken into account; if the engine be an electric motor, the source of the current, such as the battery, must also be included in the weight if the motor is to be considered complete.

We may, of course, drop the water motor out of the question, and this will then leave for consideration the piston and cylinder, or the rotary engine operated by the pressure of some kind of gas, and the electric motor; the reaction system, as we have seen in the chapter on Motive Instruments, being worthless.

The characteristics of any piston and cylinder engine which shall develop great energy in proportion to its weight will be high-cylinder pressures per square inch and high-piston speed in feet per minute, and also high economy, for the weight of the supplies of the engine must be reckoned with the total weight.

The power developed by any engine will be in proportion to the total pressure upon the piston multiplied by the speed of the piston, so it is evident that the engine which will develop the greatest energy in proportion to its weight will be the one which will have the highest rate of piston speed.

If the cylinder be merely proportioned to resist the bursting strain it will theoretically remain at the same weight for all pressures employed; but it cannot be so proportioned, and the frame necessary to hold a large cylinder, even if it be of the same weight as a small one, will be heavier than that required to hold the latter. The element of stiffness must be considered —the internal friction of a large piston is greater than that of a small one, etc.—so that the engine with the high pressure and the correspondingly small cylinder will be much smaller and lighter than the engine with the large cylinder and the low pressure.

The thoughts of many will turn to the gas or gasoline engine as being most available for the purposes of aerial navigation, for the reason that no special apparatus, such as a boiler, is required to develop the pressure; that it is unnecessary to carry any special fluid, such as water, to act as a medium for turning heat into mechanical motion, and that the fuel may be carried in a most convenient and compact form, such as gasoline, which will create equal energy with one-third of the fuel required by steam. But the gas or gasoline engine, as built at present, or in any modification of its present form, will be too heavy for the purposes of aerial navigation.

The greatest trouble with the gas engine is the extremely low pressures at which it is obliged to work; and although no special apparatus is needed to develop and store the pressure required, as in a steam engine, still, unless such a device is used, the pressure will not act upon the piston almost continuously, as is the case with the steam engine, but will only operate on the piston for one-half or perhaps one-quarter of the time.

The fundamental trouble with the gas engine in its inability to develop high pressures is the fact that the 1 cub. ft. of the mixture of gas and air which is exploded will make less than 1 cub. ft. of resultants of explosion if those resultants are reduced to the same temperature and atmospheric pressure—that is to say, if a cubic foot of a proper mixture of gas and air be exploded at atmospheric pressure and temperature, and the resultant gases of the explosion be cooled down to the same temperature at which they were exploded and placed under the same (atmospheric) pressure, they will occupy less space than the original mixture of gas and air did. But the gases after explosion are at a much higher temperature than they were before, so that the resultants of explosion immediately after that action occupy a considerably greater space than the original mixture did. The pressure is due to the fact that the gases being heated, occupy the greater space, and since they are confined to their original bulk, the pressure they exert will be in proportion to the temperature.

This temperature is, of course, limited, for a given quantity of a mixture of gas and air when burned are only capable of producing a given quantity of heat, and this given quantity of heat will not heat the resultants of explosion above a certain temperature. In the ordinary gas engine this temperature will be somewhere in the neighborhood of 2,500° F., and the pressure per square inch which will result, if the resultant gases are compelled to occupy the same space as the component ones, will be only about 75 lbs. above the atmospheric pressure; and since the pressure will decrease as the gases expand behind the piston, the mean pressure throughout the stroke of the engine will be only about 40 lbs. per square inch.

In an engine such as this it is only during one-half of the stroke of the piston that there is any pressure at all, for during the first half the inflammable mixture is being drawn into the cylinder. Now, the improvement which has greatly increased the efficiency of the gas engine is to compress the mixture of gas and air before explosion; but to do this requires a special pump or else one stroke of the engine must be sacrificed, during which time it acts as a pump to compress the mixture of gas and air. There is, of course, a considerable heat developed by the explosion, and therefore special means must be taken to keep the working parts of the gas engine cool; this is done by water, the cylinder being jacketed so that it may be cooled by circulation of water around it. For this reason it has been found impracticable to use more than one end of the cylinder for working purposes. If the cylinder be enclosed at each end and the charges of gas and air exploded in each end, the piston becomes so hot as to prevent the operation of the machine. All of these mechanical difficulties can probably be overcome; but the limited pressures which can be employed, or the special means which must be employed if higher pressures are to be used, probably warrant the statement that the gas engine in its present form will never become the lightest prime mover for the energy it develops, and also that it can

probably never be made light enough for the purposes of aerial navigation.

Although the gas engine will never probably be the lightest prime mover, still it may be that an explosive engine of some kind will be.

Nitro-glycerine will, when exploded, create an enormous mass of gas, and here is an opportunity to obtain almost unlimited pressures; but the difficulties of developing such an engine would be so great that the writer certainly would not feel like devoting a lifetime to the work—a lifetime, too, which might be very considerably and suddenly abbreviated by some kind of premature explosion. Owing to the character of this explosive, it is impossible to predict what will be the result of any particular explosion. The same quantity of explosive may give two or three times the volume of resulting gases at one time as at another, according to the circumstances. This would seem to indicate that a separate apparatus or explosion chamber of some kind would be required wherein a great number of small explosions would be used to work up the pressure, so that the effect of any one of the explosions would make no material difference, no matter what it might be. As in the gas engine, some means would have to be adopted to keep the parts of the engine exposed to the heat at a reasonable temperature. The element of danger might be obviated, for there are explosives of the Sprengel class ("helofite," for example), which are composed of two liquids that are absolutely non explodable until they are mixed, so that it is seen that there is a possibility that the apparatus might be devised in such a way as to mix these materials just previous to explosion, and in this way there might be so little of the explosive on hand at any one time that it all might explode without doing damage. It is doubtful if such an engine will be developed during the life of this generation, if at all, but it is well enough to point out the possibility of such an engine.

The steam engine has been so studied and experimented upon that it is almost impossible to tell how light such an engine of any given energy will be before it is put in operation. The great advantage that the steam-engine has over any explosive engine is that the pressure may be made anything desired, and that that pressure is always perfectly under control, and also that no special means for modifying the temperature of the working parts of the engine are required. Some of the disadvantages are its wastefulness of fuel, as compared with the gasoline engine, and the fact that a special apparatus of considerable weight (the boiler) is required to develop the pressure, and a special fluid (water) is required to transmute the heat into mechanical motion. Both the boiler and the water are important elements in the weight of the engine; so important are they that they are by a very considerable proportion the largest part of the weight of the complete motor. In developing the steam engine, therefore, into a light motor, more particular attention should be given to the design of the boiler; and, fortunately, that apparatus is capable of being made very much lighter than it is usually constructed.

The lightest form of boiler in practical use at present is on the small steam yachts, and is of the kind known as the "water tube." In these boilers the water is inside of small tubes, and the flame from the furnace, which in some cases is merely a row of gasoline jets, plays directly upon the exterior of the tubes.

The capability of any boiler to create steam will in general depend upon the extent of the surface which is exposed to the heat; and the advantage of making these water tubes very small, if a light boiler is to be made, is easily seen.

If the tube be 1 in. in diameter, and be proportioned to resist a certain bursting strain, it will be, say, $\frac{1}{32}$ in. thick and $3\frac{1}{7}$ in. in circumference. The weight will be the thickness multiplied by the circumference multiplied by the length in inches, and multiplied by the weight of the metal per cubic inch. The heating surface will be the circumference multiplied by the length. Now, suppose that the diameter of the tube be reduced to $\frac{1}{2}$ in., the thickness of the metal required to resist the same bursting strain as before will only be $\frac{1}{64}$ in. instead of $\frac{1}{32}$ in., and the circumference of the tube will only be $1\frac{4}{7}$ in.; and if the length remain the same, the weight will be just one-fourth of that of the tube of 1 in. diameter. The heating surface, however, has been decreased by one-half, for it is the length of the tube multiplied by the circumference. It is seen, then, that by decreasing the diameter of the tube by one-half the weight has been decreased by 4 and the heating surface by 2; the proportion between the heating surface and the weight has, therefore, been increased by 2; there is twice the heating surface for the same weight as there was before. The other advantages, such as the ability to concentrate the heat (for there will be less "cooling" surface with a small-tubed boiler than with a large one), and the fact that the heat of the furnace is brought so much closer to the water when thin tubes are used, are advantages which are evident, but which are difficult to indicate mathematically.

There is, of course, a limit to this decrease in the size of the boiler tubes. In the second case there will be half as much water converted into steam in the same length of time as in the former, for the heating surface has been decreased by 2; but this one-half quantity of water must flow through a tube of only one-fourth the sectional area which the larger tube possessed. An indefinite decrease in the size of the water tubes would therefore bring us to a point where the tube would be so small that the water would be unable to flow through it rapidly enough to generate the quantity of steam of which the heating surface was capable.

In regard to the weight of the water itself it may be said that an aerial apparatus driven by a steam engine *must* be so arranged that the steam can be condensed and the water used repeatedly. This is probably a matter easy of accomplishment. The great surfaces of the aeroplane will offer an opportunity for the construction of a condenser which will be more than ample; but even if such surfaces be more than sufficient, a much greater surface than is necessary should be used, for

the more rapidly the steam is turned into water the more frequently can the water be used and the less of it will have to be carried.

In regard to the engine itself it may be said that, in order to secure lightness of the boiler, it may be necessary to make the engine somewhat heavier than would otherwise be the case. By compounding the engine, using the steam in one cylinder after it has been exhausted from another, a very considerable economy both in steam and in fuel can be effected, so much so, in fact, that the additional material which is so used in the engine can probably be much more than saved in the weight of the boiler and in the fuel which is to be employed.

For the fuel of such an engine oil will certainly be selected on account of its easy transportation, and also on account of its high efficiency as regards weight when compared with coal. One pound of oil will do about the same work in boiling water as 2 lbs. of coal; the application of its heat in the form of a gasoline jet is very effective, and the furnace, if it can be called such, will be of great lightness.

As to the actual weight, or, rather, lightness, at which steam engines can be manufactured, there is considerable doubt. Until a year or two ago, when Mr. Maxim undertook to build an aerial vessel to be operated by steam power, few special attempts had been made to build a powerful steam engine and build it extremely light. Mr. Maxim states that he has succeeded in constructing a compound engine with water-tube boiler and gasoline furnace to weigh at the rate of about 8 lbs. per *HP*. Up to the time of this achievement the lightest engines in use were on the steam yachts, but the total weight of such engines, including the boiler, was about 40 lbs. per *HP*., which is plainly too heavy for the purposes of aerial navigation.

But though the engine and boiler may be constructed so as to be of very light weight, there is the element of the water which must be used to produce steam that must be taken into consideration. The amount of water that is usually vaporated in the operation of steam engines may be roughly stated as being 25 lbs. per *HP*. per hour, so that under the circumstances under which an aerial steam engine would operate, it is plain that this water must be condensed so as to be used repeatedly, or the voyage would be of an excessively brief duration. The amount of water which will be required for the operation of the condensing aerial steam engine will be dependent upon the rapidity with which the steam can be condensed. If it is going to take an hour to condense the steam, it is plain that the amount of water that will be required would be 25 lbs. per *HP*., which is clearly more than can be allowed. It is seen, therefore, that in this construction the amount of water that will have to be employed in the condensing engine can be considered as a part of the motor itself, for it will be used over and over again, and the only supply required will be that necessary to make good unavoidable losses caused by the leakage of either water or steam. Under the circumstances the

only way in which the steam can be condensed is by enclosing it within surfaces exposed to the air, and the extent of these surfaces will determine the rapidity with which the steam can be condensed. The whole 1,000 sq. ft. of supporting aeroplane surface that has been estimated in preceding chapters will provide 2,000 sq. ft. of condensing surface, for both top and bottom sides may be used. These 2,000 sq. ft. will condense 3,000 lbs. of steam per hour; and since the *HP.* that was required to operate the apparatus was, say, 40 *HP.*, the engine will require $40 \times 25 = 1,000$ lbs. of water per hour. It is seen that the aeroplane will not only condense the steam as fast as it is formed, but that it will condense it much faster. If it will condense the steam just as fast as it is formed, it will be fast enough, for in that case the amount of water required will be as small as possible. All the water that will be required will be such as to supply the boiler with water and steam and the cylinders and condenser with steam.

Although the rotary engine, as it is commonly known, presents no advantages over the reciprocating engine in the problem of constructing a light motor, and many disadvantages, still there is a form of rotary steam engine which may be developed into one that will be suitable for the purposes of aerial navigation. This is the steam turbine. This motor is made on the principle of the turbine water wheel; but steam used in this way has one great advantage over water—it can be used a number of times. In the case of a turbine water wheel, when the fluid has struck the blades of the wheel, and its velocity has been checked, its usefulness is past, and it is permitted to escape as freely as is possible; but in the case of the steam turbine the fluid may be used many times. This fact is due to the expansion of the steam. When it has struck one set of blades, and its velocity has been checked, it is permitted to strike a succeeding set of blades, and the expansion of the gas gives it the necessary velocity for doing further work. One of the forms of the steam turbine closely resembles a series of concentric turbine water wheels, each alternate set of blades being movable, the other sets being fixed. The moving blades are supported from a common shaft, the steam is admitted at the center and finds its escape between the blades to the outside in radial directions.

The turbine itself is a very light engine in proportion to the power it will furnish. It is commonly made as light as 4 or 5 lbs. per *HP.*, and can undoubtedly be made so as to develop an *HP.* of energy for each 2 lbs. weight of the turbine; but unfortunately it is at the present state of its development very extravagant of steam, probably requiring in the neighborhood of 50 per cent. more steam for the same work than is required by the reciprocating engine. This means a boiler that is 50 per cent. heavier per *HP.* than is required for the reciprocating engine, and also 50 per cent. more fuel and water.

When this is taken into consideration it will be found that not only will the complete steam turbine motor be heavier per *HP.* than the complete steam reciprocating motor, but that the weight of the boiler and of the fuel that would be required

for a brief voyage with the turbine would be greater than the weight of the complete reciprocating motor prepared for an equally long trial.

The extreme lightness of the steam turbine is due to the remarkable speed with which it revolves. Speeds of 35,000 revolutions per minute can be obtained with a turbine of a few inches diameter without difficulty and without causing trouble in operation. It is probable that 10,000 revolutions is about as slowly as it can be operated without impairing its efficiency. Of course such speeds would have to be reduced by some kind of gearing before the work could become useful, and the weight of this gearing would have to be taken into consideration in estimating the complete motor.

Notwithstanding the fact that the steam turbine in its present state of development does not seem to satisfy the requirements of aerial navigation, it is possible that its future development may be such as to warrant its adoption.

There is also a possibility of adapting the turbine so that it may perhaps be the lightest of all motors. If in the place of steam pressure the turbine were driven by pressure developed from the explosion of gas and air, as in the case of the gas engine, the motor would be lightened by the weight of the boiler, the water, and quite a large proportion of the necessary fuel. In the place of the boiler, however, it would probably be necessary to have some kind of apparatus, a sort of explosion chamber, for developing the necessary pressure, though such apparatus would be of small weight compared with the boiler that the steam engine would require; and it would also be necessary to construct the turbine with some kind of circulatory system and to provide the necessary circulating water to keep the moving parts cool. The pressures in such a machine would be much less than those employed in the steam engine —possibly the mean pressure would only be one-third as much as in the latter; this would mean that the turbine would have to be three times as heavy as would be the case if steam were used, or that it would weigh, perhaps, 6 lbs. per *HP*.

For a mere tentative experiment the use of electricity offers the most promising prospects of immediate results. Although an apparatus operated by such power would not be an independent flying machine, it would so plainly indicate the requirements that a motor which would operate the independent flying machine must meet, that nearly all the inventors of the world would then endeavor to solve a problem that would be so plainly stated.

The complete electric motor includes the source of the electric current, whether battery or dynamo. Were the motor to be operated by battery current, as has been done in the cases of two of the navigable balloons that have been constructed, it would be an exceedingly heavy instead of an exceedingly light motor. The primary battery is lighter than the storage battery for the energy that it will give out, and the primary battery and motor of the French war balloon *La France*, which was constructed with especial view to lightness, weighed at the rate of 130 lbs. per *HP*. per hour.

But, unlike all other motors, it is possible to almost completely separate the two essential parts of the electric motor—the source of the current and the motor proper—and not only to separate them, but to move them with relation to each other as much as may be desired and at any reasonable speed. There would probably be little difficulty in arranging trolleys and wires so that the current which should operate the electric motor would be taken from these wires and conveyed by conductors to the apparatus above them, the whole being somewhat similar to the present electric railway practice.

There have probably been but few attempts to make an excessively light electric motor. The writer knows of but one in which M. G. Trouvé, of Paris, succeeded in constructing a very small motor which developed energy at the rate of a *HP.* for each 7 lbs. of weight, and which raised itself vertically by means of an aerial screw. The street-car motor, which, while not constructed with the especial end of lightness in view, still is not overloaded with material, will weigh less than 40 lbs. per *HP.* exclusive of frame. The weighty materials essential in the motor are the copper for carrying the electric circuit and the iron for carrying the magnetic current. If aluminum be substituted for copper the weight of the electric circuit will be reduced by about one-half, and if pure nickel be substituted for iron the weight of the magnetic circuit will also be reduced.

It would seem as if in the present state of knowledge that the electric motor would be the easiest of all to develop to the required point of lightness. For although steam and explosive engines undoubtedly show possibilities of such development, the work would be expensive and arduous and might be futile. But with the present knowledge that we possess of electricity it would appear that a sufficiently light motor was in sight to operate a quasi-flying machine.

If an inventor, therefore, believes that he has conceived a design which will meet the other conditions that have been mentioned as necessary to observe for a flying machine, and is restrained by the difficulty of securing a sufficiently light motor, he may make tentative experiments by providing his apparatus with a light dynamo and connecting it by a wire with a source of electrical energy remaining on the ground.

My purpose will have been fulfilled if I have succeeded in indicating to him the various conditions to be met and the various calculations to be made.

THE INTERNAL WORK OF THE WIND.*

BY S. P. LANGLEY.

PART I.—INTRODUCTORY.

IT has long been observed that certain species of birds maintain themselves indefinitely in the air by "soaring," without

* This paper was read by title to the National Academy of Sciences in April, 1893, and in full before the International Conference on Aerial Navigation, at Chicago, in August, 1893.

any flapping of the wing or any motion other than a slight rocking of the body; and this, although the body in question is many hundred times denser than the air in which it seems to float with an undulating movement, as on the waves of an invisible stream.

No satisfactory mechanical explanation of this anomaly has been given, and none would be offered in this connection by the writer were he not satisfied that it involves much more than an ornithological problem, and that it points to novel conclusions of mechanical and utilitarian importance. They are paradoxical at first sight, since they imply that under certain specified conditions very heavy bodies, entirely detached from the earth, immersed in and free to move in the air, can be sustained there indefinitely without any expenditure of energy from within.

These bodies may be entirely of mechanical construction, as will be seen later, but for the present we will continue to consider the character of the invisible support of the soaring bird, and to study its motions, though only as a pregnant instance offered by nature, to show that a rational solution of the mechanical problem is possible.

Recurring, then, to the illustration referred to in the first paragraph, we may observe that the flow of an ordinary river would afford no explanation of the fact that nearly inert creatures, while free to move, although greatly denser than the fluid, yet float upon it; which is what we actually behold in the aerial stream, since the writer, like others, has satisfied himself by repeated observation that the soaring vultures and other birds appear as if sustained by some invisible support in the stream of air, sometimes for at least a considerable fraction of an hour. It is frequently suggested by those who know these facts only from books that there must be some quivering of the wings, so rapid as to escape observation. Those who do know them from observation are aware that it is absolutely certain that nothing of the kind takes place, and that the birds sustain themselves on pinions which are quite rigid and motionless, except for a rocking or balancing movement involving little energy.

The writer desires to acknowledge his indebtedness to that most conscientious observer, M. Mouillard,* who has described these actions of the soaring birds with incomparable vividness and minuteness, and who asserts that they under certain circumstances, without flapping their wings, rise and actually advance against the wind.

To the writer, who has himself been attracted from his earliest years to the mystery which has surrounded this action of the soaring bird, it has been a subject of continual surprise that it has attracted so little attention from physicists. That nearly inert bodies, weighing from 5 to 10 and even more pounds, and many hundred times denser than the air, should be visibly suspended in it above our heads, sometimes for

* L. P. Mouillard, "L'Empire de l'Air." Paris: G. Masson.

hours at a time, and without falling—this, it might seem, is, without misuse of language, to be called a physical miracle; and yet the fact that those whose province it is to investigate nature have hitherto seldom thought it deserving attention is perhaps the greater wonder.

This indifference may be in some measure explained by the fact that the largest and best soarers are of the vulture kind, and that their most striking evolutions are not to be seen in those regions of the northern temperate zone, where the majority of those whose training fits them to study the subject are found. Even in Washington, however, where the writer at present resides, scores of great birds may be seen at times in the air together gliding with and against the wind, and ascending higher at pleasure on nearly motionless wings. "Those who have not seen it," says M. Mouillard, "when they are told of this ascension without the expenditure of energy, are always ready to say, 'But there must have been movements, though you did not see them;' and in fact," he adds, "the casual witness of a single instance, on reflection, himself feels almost a doubt as to the evidence of his senses when they testify to things so extraordinary."

Quite agreeing with this, the writer will not attempt any general description of his own observations; but as an illustration of what can sometimes be seen, will give a single one, to whose exactness he can personally witness. The common turkey buzzard (*Cathartes aura*) is so plenty around the environs of Washington that there is rarely a time when some of them may not be seen in the sky, gliding in curves over some attractive point or (more rarely) moving in nearly straight lines on rigid wings, if there be a moderate wind. On the only occasion when the motion of one near at hand could be studied in a very high wind, the author was crossing the long Aqueduct Bridge over the Potomac in an unusually violent November gale, the velocity of the wind being probably over 35 miles an hour. About one-third of the distance from the right bank of the river, and immediately over the right parapet of the bridge, at a height of not over 20 yds., was one of these buzzards, which, for some object which was not evident, chose to keep over this spot, where the gale, undisturbed by any surface irregularities, swept directly up the river with unchecked violence. In this aerial torrent, and apparently indifferent to it, the bird hung, gliding in the usual manner of its species, round and round, in a small oval curve, whose major axis (which seemed toward the wind) was not longer than twice its height from the water. The bird was therefore at all times in close view. It swung around repeatedly, rising and falling slightly in its course, while keeping, as a whole, on one level and over the same place, moving with a slight swaying, both in front and lateral direction, but in such an effortless way as suggested a lazy yielding of itself to the rocking of some invisible wave.

It may be asserted that there was not only no flap of the wing, but not the quiver of a wing feather visible to the

closest scrutiny during the considerable time the bird was under observation, and during which the gale continued. A record of this time was not kept, but it at any rate lasted until the writer, chilled by the cold blast, gave up watching and moved away, leaving the bird still floating about, at the same height in the torrent of air, in nearly the same circle, and with the same aspect of indolent repose.

If the wind is such a body as it is commonly supposed to be, it is absolutely impossible that this sustentation could have taken place in a horizontal current any more than in a calm; and yet that the ability to soar is, in some way, connected with the presence of the wind, became to the writer as certain as any fact of observation could be, and at first the difficulty of reconciling such facts (to him undoubted) with accepted laws of motion seemed quite insuperable.

Light came to him through one of those accidents which are commonly found to occur when the mind is intent on a particular subject, and looking everywhere for a clew to its solution.

In 1887, while engaged with the "whirling-table" in the open air at the Allegheny Observatory, he had chosen a quiet afternoon for certain experiments, but in the absence of the entire calm which is almost never realized, had placed one of the very small and light anemometers made for hospital use in the open air, with the object of determining and allowing for the velocity of what feeble breeze existed. His attention was called to the extreme irregularity of this register, and he assumed at first that the day was more unfavorable than he had supposed. Subsequent observations, however, showed that when the anemometer was sufficiently light and devoid of inertia, the register always showed great irregularity, especially when its movements were noted not from minute to minute, but from second to second.

His attention once aroused to these anomalies, he was led to reflect upon their extraordinary importance in a possible mechanical application. He then designed certain special apparatus, hereafter described, and made observations with it which showed that "wind" in general was not what it is commonly assumed to be—that is, air put in motion with an approximately uniform velocity in the same strata—but that, considered in the narrowest practicable sections, wind was always not only not approximately uniform, but variable and irregular in its movements beyond anything which had been anticipated, so that it seemed probable that the very smallest part observable could not be treated as approximately homogeneous, but that even here there was an internal motion to be considered, distinct both from that of the whole body and from its immediate surroundings. It seemed to the writer to follow as a necessary consequence that there might be a potentiality of what may be called "internal work"* in the wind.

* Since the term "internal work" is often used in thermodynamics to signify molecular action, it may be well to observe that it here refers not to

On further study it seemed to him that this internal work might conceivably be so utilized as to furnish a power which should not only keep an inert body from falling, but cause it to rise, and that while this power was the probable cause of the action of the soaring bird, it might be possible through its means to cause any suitably disposed body, animate or inanimate, wholly immersed in the wind and wholly free to move, to advance against the direction of the wind itself. By this it is not meant that the writer then devised means for doing this, but that he then attained the conviction both that such an action involved no contradiction of the laws of motion, and that it was mechanically possible (however difficult it might be to realize the exact mechanism by which this might be accomplished).

It will be observed that in what has preceded it is intimated that the difficulties in the way of regarding this even in the light of a theoretical possibility may have proceeded, with others as with the writer, not from erroneous reasoning, but from an error in the premises, entering insidiously in the form of the tacit assumption made by nearly all writers, that the word "wind" means something so simple, so readily intelligible, and so commonly understood, as to require no special definition; while, nevertheless, the observations which are presently to be given show that it is, on the contrary, to be considered as a generic name for a series of infinitely complex and little known phenomena.

Without determining here whether any mechanism can be actually devised which shall draw from the wind the power to cause a body wholly immersed in it to go against the wind, the reader's consideration is now first invited to the evidence that there is no contradiction to the known laws of motion, and at any rate no theoretical impossibility in the conception of such a mechanism, if it is admitted that the wind is not what it has been ordinarily taken to be, but what the following observations show that it is.

What immediately follows is an account of evidence of the complex nature of the "wind," of its internal movements, of the resulting potentiality of this internal work, and of attempts which the writer has made to determine quantitatively its amount by the use of special apparatus, recording the changes which go on (so to speak) *within* the wind in very brief intervals. These results may, it is hoped, be of interest to meteorologists, but they are given here with special reference to their important bearing on the future of what the writer has ventured to call the science of aerodronomics.*

The observations which are first given were made in 1887

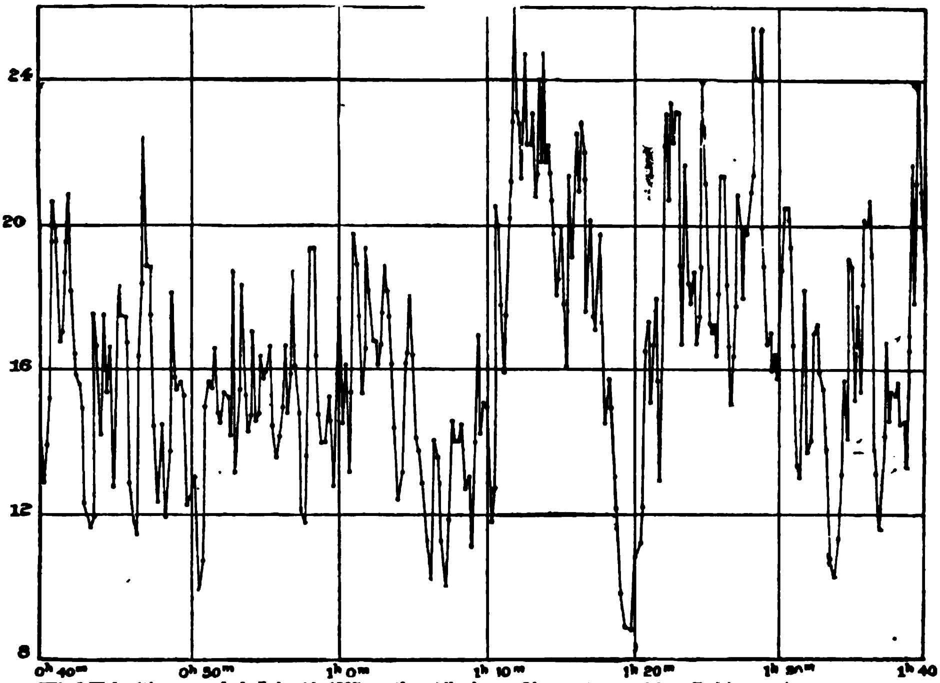
molecular movements, but to pulsations of sensible magnitude always existing in the wind, as will be shown later, and whose extent and extraordinary possible mechanical importance it is the object of this research to illustrate. The term is so significant of the author's meaning that he permits himself the use of it here in spite of the possible ambiguity.

* From αεροδρομεω, to traverse the air; αεροδρομος, an air-runner.

at Allegheny, and are supplemented by others made at Washington in the present year.*

What has just been said about their possible importance

PLATE I.



Wind Velocities recorded July 16, 1887, at the Allegheny Observatory, with a Robinson Anemometer registering every twenty-five revolutions.

Abscissæ = Time.

Ordinates = Wind Velocities in miles per hour.

will, perhaps, seem justified, if it is remarked (in anticipation of what follows later) that the result of the present discussion

* It will be noticed that the fact of observation here is not so much the movement of currents, such as the writer has since learned was suggested by Lord Rayleigh so long ago as 1883, still less of the movement of distinct currents at a considerable distance above the earth's surface, but of what must be rather called the effect of the irregularities and pulsations of any ordinary wind within the immediate field of examination, however narrow. See the instructive article by Lord Rayleigh in *Nature*, April 5, 1883. Lord Rayleigh remarks that continued soaring implies "(1) that the course is not horizontal; (2) that the wind is not horizontal, or (3) that the wind is not uniform." "It is probable," he says, "that the truth is usually represented by (1) or (2); but the question I wish to raise is whether the cause suggested by (3) may not sometimes come into operation."

implies not only the theoretical but the mechanical possibility that a heavy body, wholly immersed in the air and sustained by it, may, without the ordinary use of wind or sail or steam, and without the expenditure of any power except such as may be derived from the ordinary winds, make an aerial voyage in any direction whose length is only limited by the occurrence of a calm. A ship is able to go against a head wind by the force of that wind, owing to the fact that it is partly immersed in the water, which reacts on the keel; but it is here asserted that (contrary to usual opinion and in opposition to what at first may seem the teachings of physical science) it is not impossible that a heavy and nearly inert body *wholly* immersed in the air can be made to do this.

The observations on which the writer's belief in this mechanical possibility are founded will now be given.

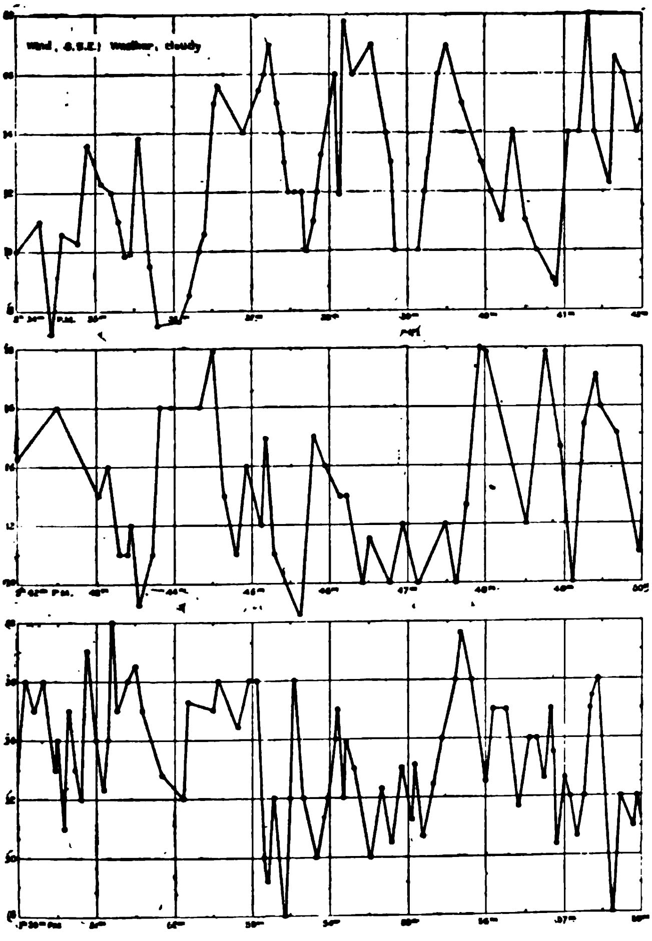
In the ordinary use of the anemometer (let us suppose it to be a Robinson's anemometer, for illustration) the registry is seldom taken as often as once a minute; thus, in the ordinary practice of the United States Weather Bureau, the registration is made at the completion of the passage of each mile of wind. If there be very rapid fluctuations of the wind, it is obviously desirable in order to detect them to observe the instrument at very brief intervals—*e.g.*, at least every second, instead of every minute or every hour—and it is equally obvious that, in order to take up and indicate the changes which occur in these brief intervals, the instrument should have as little inertia as possible, its momentum tending to falsify the facts by rendering the record more uniform than would otherwise be the case.

In 1887 I made use of the only apparatus at command, an ordinary small Robinson's anemometer, having cups 3 in. (7.5 cm.) in diameter, the center of the cups being 6½ in. (16½ cm.) from the center of rotation. This was placed at the top of a mast 53 ft. (16.2 m) in height, which was planted in the grounds of the Allegheny Observatory on the flat summit of a hill which rises nearly 400 ft. (122 m.) above the valley of the Ohio River. It was, accordingly, in a situation exceptionally free from those irregularities of the wind which are introduced by the presence of trees and of houses or of inequalities of surface.

Every twenty-fifth revolution of the cups was registered by closing an electric circuit, and the registry was made on the chronograph of the observatory by a suitable electric connection, and these chronograph sheets were measured and the results tabulated. A portion of the record obtained on July 16, 1887, is given on Plate I, the abscissæ representing time, and the ordinates wind velocities. The observed points represent the wind's velocities as computed from the intervals between each successive electrical contact, as measured on the chronograph sheets, and for convenience in following the succession of observed points they are here joined by straight lines, though it is hardly necessary to remark that the change in velocity is in fact, though quite sharp, yet not in general dis-

continuous, and the straight lines here used for convenience do not imply that the rate of change of velocity is uniform.

PLATE II.



PART II.—EXPERIMENTS WITH THE USE OF SPECIAL APPARATUS.

The wind velocities during this period of observation ranged from about 10 to 25 miles an hour, and the frequency of

measurement was every 7 to 17 seconds. If, on the one hand, owing to the weight and inertia of the anemometer, this is far from doing justice to the actual irregularities of the wind, on the other, it equally shows that the wind was far from being a body of even approximate uniformity of motion, and that even when considered in quite small sections the motion was found to be irregular almost beyond conception, certainly beyond anticipation; for this record is not selected to represent an extraordinary breeze, but the normal movement of an ordinary one.

By an application of these facts, to be presented later, I then reached by these experiments the conclusion that it was theoretically possible to cause a heavy body wholly immersed in the wind to be driven in the opposite direction—*e.g.*, to move east while the wind was blowing west, without the use of any power other than that which the wind itself furnished, and this even by the use of plane surfaces, and without taking advantage of the more advantageous properties of curved ones.

This power, I further already believed myself warranted by these experiments in saying, could be obtained by the movements of the air in the horizontal plane alone, even without the utilization of currents having an upward trend; but I was obliged to turn to other occupations, and did not resume these interesting observations until the year 1893.

Although the anemometer used at Allegheny served to illustrate the essential fact of the rapid and continuous fluctuations of even the ordinary and comparatively uniform wind, yet owing to the inertia of the arms and cups, which tended to equalize the rate (the moment of inertia was approximately 40,000 gr. cm.^{2}), and to the fact that the record was only made at every twenty-fifth revolution, the internal changes in the horizontal component of the wind's motion, thus representing its potential work, were not adequately recorded.

In January, 1893, I resumed these observations at Washington with apparatus with which I sought to remedy these defects, using as a station the roof of the north tower of the Smithsonian Institution building, the top of the parapet being 142 ft. (43.3 m.) above the ground, and the anemometers, which were located above the parapet, being 153 ft. (46.7 m.) above the ground. I placed them in charge of Mr. George E. Curtis, with instructions to take observations under the conditions of light, moderate, and high winds. The apparatus used was, first, a Weather Bureau Robinson anemometer of standard size, with aluminum cups. Diameter to center of cups, 34 cm.; diameter of cups, 10.16 cm.; weight of arms and cups, 241 gr.; approximate moment of inertia, 40,710 gr. cm.^{2}

A second instrument was a very light anemometer, having paper cups of standard pattern and diameter, the weight of arms and cups being only 74 gr., and its moment of inertia 8,604 gr. cm.^{2}

With this instrument a number of observations were taken,

when it was lost by being blown away in a gale. It was succeeded in its use by one of my own construction, which was considerably lighter. This was also blown away. I afterward employed one of the same size as the standard pattern, weighing 48 gr., having a moment of inertia of 11,940 gr. cm.², and finally I constructed one of one-half the diameter of the standard pattern, employing cones instead of hemispheres, weighing 5 gr., and having a moment of inertia of but 300 gr. cm.²

In the especially light instruments the electric record was made at every half revolution on an ordinary astronomical chronograph, placed upon the floor of the tower, connected with the anemometers by an electric circuit. Observations were made on January 14, 1893, during a light wind having a velocity of from 9 to 17 miles an hour; on January 25 and 26, during a moderate wind having a velocity of from 16 to 28 miles an hour; and on February 4 and 7, during a moderate and high wind ranging from 14 to 36 miles an hour. Portions of these observations are given on Plates II, III, and IV. A short portion of the record obtained with the standard Weather Bureau anemometer during a high northwest wind is given on Plate V.

A prominent feature presented by these diagrams is that the higher the absolute velocity of the wind, the greater the relative fluctuations which occur in it. In a high wind the air moves in a tumultuous mass, the velocity being at one moment perhaps 40 miles an hour, then diminishing to an almost instantaneous calm, and then resuming.*

The fact that an absolute local calm can momentarily occur during the prevalence of a high wind was vividly impressed upon me during the observations on February 4, when, chancing to look up to the light anemometer, which was revolving so rapidly that the cups were not separately distinguishable, I saw them completely stop for an instant, and then resume their previous high speed of rotation, the whole within the fraction of a second. This confirmed the suspicion that the chronographic record, even of a specially light anemometer, but at most imperfectly notes the sharpness of these internal changes. Since the measured interval between two electric contacts is the datum for computing the velocity, an instantaneous stoppage, such as I accidentally saw, will appear on the record simply as a slowing of the wind, and such very significant facts, as that just noted, will be necessarily slurred over even by the most sensitive apparatus of this kind. However, the more frequent the contacts, the more nearly an exact record of the fluctuations may be measured, and I have, as I have stated, provided that they should be made at every half revolution of the anemometer—that is, as a rule, several times a second.†

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* An example of a very rapid change may be seen on Plate IV at 12.23 P.M.

† Here we may note the error of the common assumption that the ordinary anemometer, however heavy, will, if frictionless, correctly measure the velocity of the wind, for the existence of "vis inertiæ," it is now seen,

I now invite the reader's attention to the actual records of rapid changes that take place in the wind's velocity, selecting as an illustration the first five and one-half minutes of the diagram plotted on Plate III.

The heavy line through points *A*, *B*, and *C* represents the ordinary record of the wind's velocity, as obtained from a standard Weather Bureau anemometer during the observations recording the passage of two miles of wind. The velocity, which was at the beginning of the interval considered nearly 23 miles an hour, fell during the course of the first mile to a little over 20 miles an hour. This is the ordinary anemometric record of the wind at such elevations as this (47 m.) above the earth's surface, where it is free from the immediate vicinity of disturbing irregularities, and where it is popularly supposed to move with occasional variation in direction, as the weather-cock indeed indicates, but with such nearly uniform movement that its rate of advance is, during any such brief time as two or three minutes, under ordinary circumstances approximately uniform. This, then, may be called the "wind"—that is, the conventional "wind" of treatises upon aerodynamics, where its aspect as a practically continuous flow is alone considered. When, however, we turn to the record made with the especially light anemometer, at every second of this same wind, we find an entirely different state of things. The wind, starting with the velocity of 23 miles an hour, at 12 hours, 10 minutes, 18 seconds, rose within 10 seconds to a velocity of 33 miles an hour, and within 10 seconds more fell to its initial speed. It then arose within 30 seconds to a velocity of 36 miles an hour and so on, with alternate rising and fallings, at one time actually stopping; and, as the reader may easily observe, passing through 18 notable maxima and as many notable minima, the average interval from a maximum to a minimum being a little over 10 seconds, and the average change of velocity in this time being about 10 miles an hour. In the lower left-hand corner of Plate III is given a conventional representation of these fluctuations in which this average period and amplitude are used as a type. The above are facts, the counterpart of which may be noted by any one adopting the means the writer has employed. It is hardly necessary to observe that almost innumerable minor maxima and minima presented themselves which the drawing cannot depict.

In order to insure clearness of perception, the reader will bear in mind that the diagram does not represent the velocities

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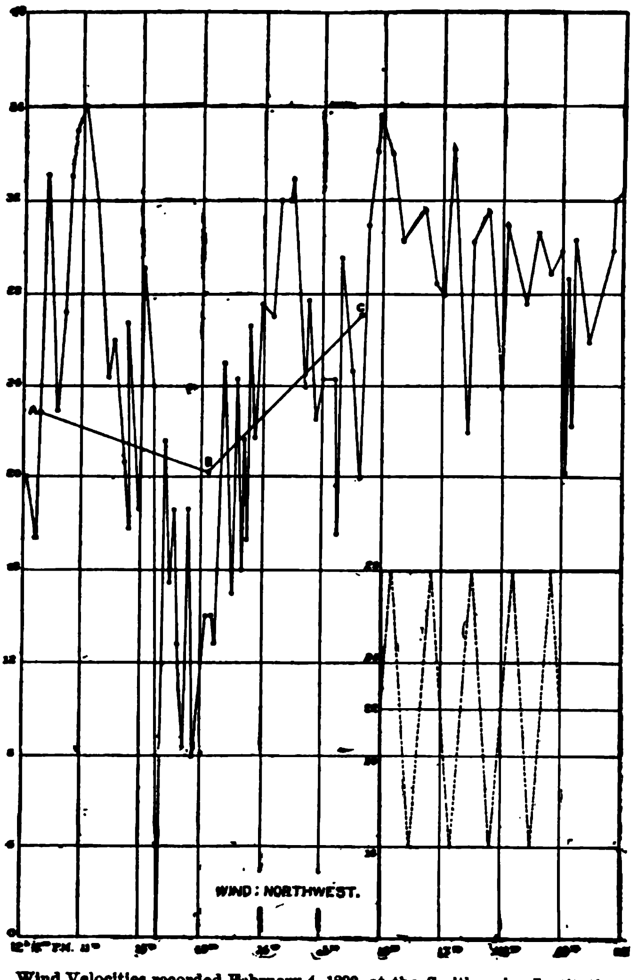
is not indifferent, but plays a most important part where the velocity suffers such great and frequent changes as we here see it does, and where the rate at which this inertia is overcome and this velocity changed is plainly a function of the density of the fluid, which density, we also see reason to suppose, itself varies incessantly and with great rapidity. Though it is probable that no form of barometer in use does justice to the degree of change of this density, owing to this rapidity, we cannot, nevertheless, suppose it to exceed certain limits, and we may treat the present records, made with an anemometer of such exceptional lightness, as being comparatively unaffected by these changes in density, though they exist.

which obtained coincidentally along the length of 2 miles of wind represented, nor the changes in velocity experienced by a single moving particle during the given interval, but that it

PLATE III.



Wind Velocities recorded February 4, 1893, at the Smithsonian Institution, with a light Robinson Anemometer (Paper Cups) registering every revolution.

is a picture of the velocities which were in this wind at the successive instants of its passing the fixed anemometer, which velocities, indeed, were probably nearly the same for a few seconds before and after registry, but which incessantly passed into and were replaced by others in a continuous flow of change. But although the observations do not show the actual changes of velocity which any given particle experiences in any assigned interval, these fluctuations cannot be materially different in character from those which are observed at a fixed point, and are shown in the diagram. It may, perhaps, still further aid us in fixing our ideas to consider two material particles as starting at the same time over this 2-mile course; the one moving with the uniform velocity of 22.6 miles an hour (33 ft. per second), which is the average velocity of the wind as observed for the interval between 12 hours, 10 minutes, 18 seconds, and 12 hours, 15 minutes, 45 seconds on February 4; the other, during the same interval, having the continuously changing velocities actually indicated by the light anemometer, as shown on Plate III. Their positions at any time may, if desired, be conveniently represented in a diagram, where the abscissa of any point represents the elapsed time in seconds, and the ordinates show the distance in feet of the material particle from the starting-point. The path of the first particle will thus be represented by a straight line, while the path of the second particle will be an irregularly curved line, at one time above and at another time below the mean straight line just described, but terminating in coincidence with it at the end of the interval. If, now, all the particles in 2 miles of wind were simultaneously accelerated and retarded in the same way as this second particle—that is, if the wind were an inelastic fluid and moved like a solid cylinder—the velocities recorded by the anemometer would be identical with those that obtained along the whole region specified. But the actual circumstances must evidently be far different from this, since the air is an elastic and nearly perfect fluid, subject to condensation and rarefaction. Hence the successive velocities of any given particle (which are in reality the resultant of incessant changes in all directions) must be conceived as evanescent, taking on something like the sequence recorded by these curves a very brief time before this air reached the anemometer, and losing it as soon after.

It has not been my purpose in this paper to enter upon any inquiry as to the cause of this non-homogeneity of the wind. The irregularities of the surface topography (including buildings and every other surface obstruction) are commonly adduced as a sufficient explanation of the chief irregularities of the surface wind; yet I believe that a considerable distance above the earth's surface (*e.g.*, 1 mile) the wind may not even be approximately homogeneous, nor have an even flow; for while, if we consider air as an absolutely elastic and frictionless fluid, any motion impressed upon it would be preserved forever, and the actual irregularities of the wind would be results of changes made at any past time, however remote,

so long as we admit that the wind, without being absolutely elastic and frictionless, is nearly so, it seems to me that we may consider that the incessant alternations which it here appears make the " wind" are due to past impulses and changes which are preserved in it, and which die away with very considerable slowness. If this be the case, it is less difficult to see how even in the upper air and at every altitude we might expect to find local variations or pulsations not unlike those which we certainly observe at minor altitudes above the ground.*

Of these irregular movements of the wind, which take place up, down, and on every side, and are accompanied of necessity by equally complex condensations and expansions, it will be observed that only a small portion—namely, those which occur in a narrow current whose direction is horizontal and sensibly linear, and whose width is only the diameter of the anemometer—can be noted by the instruments I have here described, and whose records alone are represented in the diagrams. However complex the movement may appear, as shown by the diagram, it is then far less so than the reality, and it is probable indeed that anything like a fairly complete graphical presentation of the case is impossible.

I think that on considering these striking curves (Plates I, II, III, IV, and V) we shall not find it difficult to admit, at least as an abstract conception, that there is no necessary violation of the principle of the conservation of energy implied in the admission that a body wholly immersed in and moving with such a wind may derive from it a force which may be utilized in *lifting* the body, in a way in which a body immersed in the " wind" of our ordinary conception could not be lifted; and if we admit that the body may be lifted, it follows obviously that it may descend under the action of gravity from the elevated position, on a sloping path, to some distance in a direction opposed to that of the wind which lifted it, though it is not obvious what this distance is.

We may admit all this, because we now see (I repeat) that the apparent violation of law arises from a tacit assumption, which we in common with all others may have made, that the wind is an approximately homogeneously moving body, because moving as a whole in one direction. It is, on the contrary, *always*, as we see here, filled (even if we consider only movements in some one horizontal plane) with amazingly complex motions, some of which, if not in direct opposition to the main movement, are relatively so—that is, are slower, while others are faster than this main movement, so that a portion is always opposed to it.

From this, then, we may now at least see that it is plainly within the capacity of an intelligence like that suggested by

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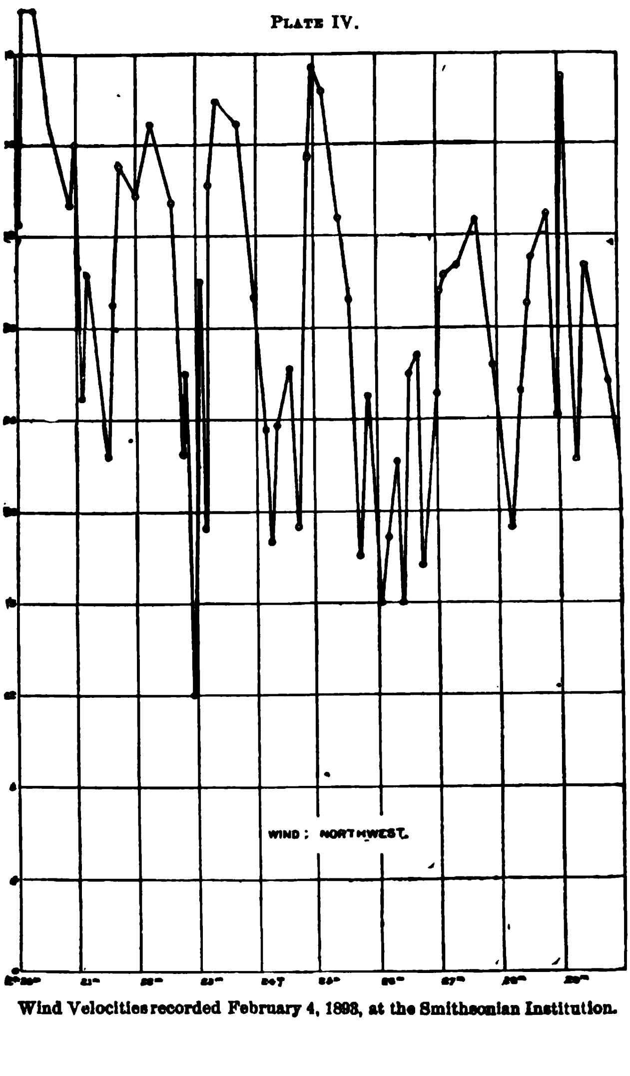
* In this connection reference may be made to the notable investigations of Helmholtz on atmospheric movements, *Sitzungsberichte*, Berlin, 1888-89.

Maxwell, and which Lord Kelvin has called the "sorting demon," to pick out from the internal motions those whose direction is opposed to the main current, and to omit those which are not so, and thus without the expenditure of energy

PLATE IV.



Wind Velocities recorded February 4, 1893, at the Smithsonian Institution.

to construct a force which will act against the main current itself.

But we may go materially further, and not only admit that it is not necessary to invoke here, as Maxwell has done in the case of thermodynamics, a being having a power and rapidity of action far above ours, but that in actual fact a being of a lower order than ourselves, guided only by instinct, may so utilize these internal motions.

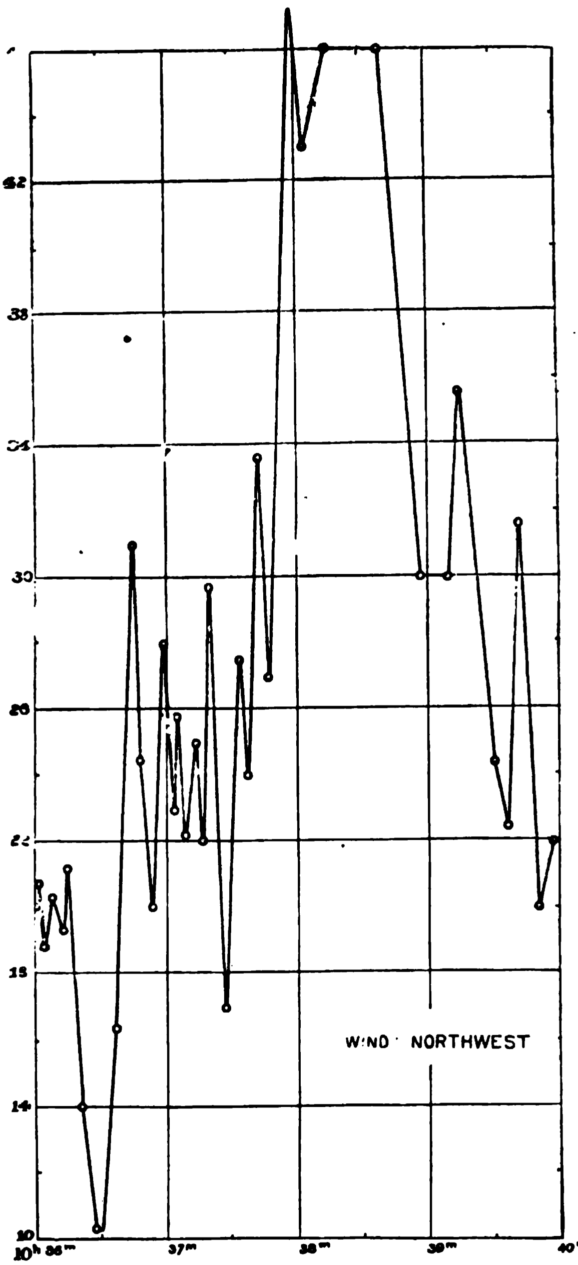
We might not indeed have conceived this possible were it not that nature has already to a large extent exhibited it before our eyes in the soaring bird,* which sustains itself endlessly in the air with nearly motionless wings, for without this evidence of the possibility of an action which now ceases to approach the inconceivable we are not likely, even if admitting its theoretical possibility, to have thought the mechanical solution of this problem possible. But although to show how this physical miracle of nature is to be imitated completely and in detail may be found to transcend any power of analysis, I hope to show that this may be possible without invoking the asserted power of "aspiration" relative to curved surfaces or the trend of upward currents, and even to indicate the probability that the mechanical solution of this problem may not be beyond human skill.

To this conclusion we are invited by the following considerations among others:

We will presently examine the means of utilizing this potentiality of internal work in order to cause an inert body, wholly unrestricted in its motion and wholly immersed in the current, to *rise*; but first let us consider such a body (a plane) whose movement is restricted to a horizontal direction, but which is free to move between frictionless vertical guides. Let it be inclined upward at a small angle toward a horizontal wind, so that only the vertical component of the pressure of the wind

* "When the condors in a flock are wheeling round and round any spot, their flight is beautiful. Except when rising from the ground, I do not recollect ever having seen one of these birds flap its wings. Near Lima I watched several for nearly half an hour without once taking off my eyes. They moved in large curves, sweeping in circles, descending and ascending without once flapping. As they glided close over my head, I intently watched, from an oblique position, the outlines of the separate and terminal feathers of the wing; and if there had been the least vibratory movement these would have blended together, but they were seen distinct against the blue sky. The head and neck were moved frequently and apparently with force, and it appeared as if the extended wings formed the fulcrum on which the movements of the neck, body, and tail acted. If the bird wished to descend, the wings for a moment collapsed, and then, when again expanded with an altered inclination, the momentum gained by the rapid descent seemed to urge the bird upward, with the even and steady movement of a paper kite. In the case of any bird *soaring*, its motion must be sufficiently rapid, so that the action of the inclined surface of its body on the atmosphere may counterbalance its gravity. The force to keep up the momentum of a body moving in a horizontal plane in that fluid (in which there is so little friction) cannot be great, and this force is all that is wanted. The movement of the neck and body of the condor we must suppose is sufficient for this. However this may be, it is truly wonderful and beautiful to see so great a bird, hour after hour, without any apparent exertion, wheeling and gliding over mountain and river."—*Darwin's "Journal of the Various Countries Visited by H. M. S. Beagle,"* pp. 223, 224.

PLATE V.



Wind Velocities observed at Smithsonian Institution, February 20, 1893, with a Robinson Anemometer (Aluminum Cups) registering every five revolutions.

Abscissæ = Time
Ordinates = Wind Velocities.

on the plane will affect its motion. If the velocity of the wind be sufficient, the vertical component of pressure will equal or exceed the weight of the plane, and in the latter case the plane will rise indefinitely.

Thus, to take a concrete example, if the plane be a rectangle whose length is six times its width, having an area of 2.3 sq. ft. to the pound, and be inclined at an angle of 7°, and if the wind have a velocity of 36 ft. per second, experiment shows that the upward pressure will exceed the weight of the plane, and the plane will rise, if between vertical nearly frictionless guides, at an increasing rate until it has a velocity of 2.52 ft. per second,* at which speed the weight and upward pressure are in equilibrium. Hence there are no unbalanced forces acting, and the plane will have attained a state of uniform motion.

For a wind that blows during 10 seconds the plane will therefore rise about 25 ft. At the beginning of the motion the inertia of the plane makes the rate of rise less than the uniform rate, but at the end of 10 seconds the inertia will cause the plane to ascend a short distance after the wind has ceased, so that the deficit at the beginning will be counterbalanced by the excess at the end of the assigned interval.

We have just been speaking of a material heavy plane permanently sustained in vertical guides, which are essential to its continuous ascent in a uniform wind; but such a plane will be lifted and sustained *momentarily*, even if there be no vertical guides; or in the case of a kite, even if there be no cord to retain it, the inertia of the body supplying for a brief period the office of the guides or of the cord. If suitably disposed, it will, as the writer has elsewhere shown, under the resistance to a horizontal wind, imposed only by its inertia, commence to move not in the direction of the wind, but nearly vertically. Presently, however, as we recognize, this inertia must be overcome, and as the inclined plane takes up more and more the motion of the wind, the lifting effect must grow less and less (that is to say, if the wind be the approximately homogeneous current it is commonly treated as being), and finally, ceasing altogether, the plane must ultimately fall. If, however, a counter-current is supposed to meet this inclined plane before the effect of its inertia is exhausted, and consequently before it

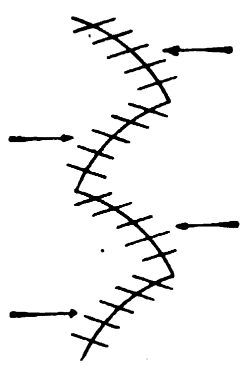


Fig. 1.

* See "Experiments in Aerodynamics, by S. P. Langley, "Smithsonian Contributions to Knowledge," 1891.

ceases to rise, we have only to suppose the plane to be rotated through 180° about a vertical axis, without any other call for the expenditure of energy, to see that it will now be lifted still higher, owing to the fact that its inertia now reappears as an active factor. The annexed sketch (fig. 1) shows a typical representation of what might be supposed to happen with a model inclined plane freely suspended in the air, and endowed with the power of rotating about a vertical axis so as to change the aspect of its constant inclination, which need involve no (theoretical) expenditure of energy, even although the plane possess inertia. We see that this plane would rise indefinitely by the action of the wind in alternate *directions*.

The disposition of the wind, which is here supposed to cause the plane to rise, appears at first sight an impossible one, but we shall next make the important observation that it becomes virtually possible by a method which we shall now point out, and which leads to a practicable one which we may actually employ.

Fig. 2 shows the wind blowing in one constant direction, but alternately at two widely varying velocities, or rather (in the extreme case supposed in illustration) where one of the velocities is negligibly small, and where successive pulsations in the same direction are separated by intervals of calm.

A frequent alternation of velocities, united with constancy of absolute direction, has previously been shown here to be the ordinary condition of the wind's motion; but attention is now particularly called to the fact that, while these unequal velocities may be in the same direction as regards the surface of the earth, yet as regards the *mean* motion of the wind they are in opposite directions, and will produce on a plane, whose inertia enables it to sustain a sensibly uniform motion with the mean velocity of this variable wind, the same lifting effect as if these same alternating winds were in absolutely opposed directions, provided that the (constant) inclination of the plane alternates in its aspect to correspond with the changes in the wind.

It may aid in clearness of conception if we imagine a set of fixed co-ordinates, $X\ Y\ Z$, passing through O, and a set of movable co-ordinates, $x\ y\ z$, moving with the velocity and in the direction of the mean wind. If the moving body is referred to these first only, it is evidently subject to pulsations which take place in the same directions on the axis of X, but it must be also evident that if referred to the second or movable co-ordinates, these same pulsations may be and are in opposite directions. This, then, is the case we have just considered, and if we suppose the plane to change the aspect* of its (constant) inclination as the direction of the pulsations changes, it is evident that there must be a gain in altitude with every pulsation, while the plane advances horizontally with the velocity of the mean wind.

* We do not for the moment consider how this change of aspect is to be mechanically effected; we only at present call attention to the fact that it involves, in theory, no expenditure of energy.

During the period of maximum wind velocity, when the wind is moving faster than the plane, the rear edge of the latter must be elevated. During the period of minimum velocity, when the plane, owing to its inertia, is moving faster than the wind, the front edge of the plane must be elevated. Thus the vertical component of the wind pressure as it strikes the oblique plane tends in both cases to give it a vertical upward thrust. So long as this thrust is in excess of the weight to be lifted, the plane will rise. The rate of rise will be greatest at the beginning of each period, when the relative velocity is greatest, and will diminish as the resistance produces "drift"—*i.e.*, diminishes relative velocity. The curved line *O B* in the vignette represents a typical path of the plane under these conditions.

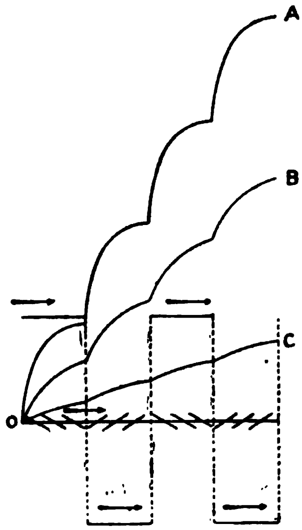


Fig. 2.

It follows from the diagram (fig. 1) that, other things being equal, the more frequent the wind's pulsations the greater will be the rise of the plane; for since during each period of steady wind the rate of rise diminishes, the more rapid the pulsations, the nearer the mean rate of rise will be to the initial rate. The requisite frequency of pulsations is also related to the inertia of the plane, as the less the inertia the more frequent must be the pulsations in order that the plane shall not lose its relative velocity.

It is obvious that there is a limit of weight which cannot be exceeded if the body is to be sustained by any such fluctuations of velocity as can be actually experienced. Above this limit of weight the body will sink. Below this limit the lighter the body is the higher it will be carried, but with increasing variability of speed. That body, then, which has the greatest weight per unit of surface will soar with the greatest steadiness, if it soar at all, not on account of this weight, *per se*, but because the weight is an index of its inertia.

The reader who will compare the results of experiments made with any artificial flying models, like those of Pénaud, with the weights of the soaring birds as given in the tables by M. Mouillard or other authentic sources, cannot fail to be struck with the great weight in proportion to wing surface which nature has given to the soaring bird, compared with

any which man has yet been able to imitate in his models. This fact of the weight of the soaring bird in proportion to its area has been again and again noted, and it has been frequently remarked that without weight the bird could not soar, by writers who felt that they could very safely make such a paradoxical statement, in view of the evidence nature everywhere gave that this weight was indeed in some way necessary to rising; but these writers have not shown, so far as I remember, how this necessity arises, and this is what I now endeavor to point out.*

It has not here been shown what limit of weight is imposed to the power of an ordinary wind to elevate and sustain, but it seems to me, and I hope that it may seem to the reader, that the evidence that there is *some* weight which the action of the wind is sufficient to permanently sustain under these conditions in a free body, has a demonstrative character, although no quantitative formula is offered at this stage of the investigation. It is obvious that, if this weight is sustainable at any height, gravity may be utilized to cause the body (which we suppose to be a material plane) to descend on an inclined course to some distance, even against the wind.

I desire, in this connection, to remark that the preceding experiments and deductions, showing that a material free plane,† possessing sufficient inertia, may in theory rise indefinitely by the action of an ordinary wind, without the expenditure of work from any internal source (as well as those statements which follow), when these explanations are once made, have a character of obviousness, which is due to the simplicity of the enunciation, but not, I think, to the familiarity of the explanation; for though attention is beginning to be paid by meteorologists to the rapidity of these wind fluctuations, I am not aware that their effects have been so exhibited, or especially that they have been presented in this connection, or that the conclusions which follow have been drawn from them.

We have here seen, then, how pulsations of sufficient amplitude and frequency, of the kind which present themselves in nature, may, in theory, furnish energy not only sufficient to sustain, but actually to elevate a heavy body moving in and with the wind at its mean rate.

It is easy to now pass to the practical case which has been already referred to, and which is exemplified in nature—

* It is, perhaps, not superfluous to recall here that, according to the researches of Rankine, Froude, and others, a body molded in wave-line curves would, if frictionless, continue to move indefinitely against an opposed wind in virtue of inertia and once acquired velocity, and also to recall how very small the effect of fluid friction in the air has been shown to be (by the writer in a previous investigation).

† I use the word "plane," but include in the statement all suitable modifications of a curved surface. I desire to recall attention to the paragraph in "Experiments in Aerodynamics," in which I caution the reader against supposing that by investigating plane surfaces I imply that they are the best form of surface for flight; and I repeat here that, as a matter of fact, I do not believe them to be so. I have selected the plane simply as the best form for preliminary experiment.

namely, that in which the body (*e.g.*, the bird soaring on rigid wings, but having power to change its inclination) uses the elevation thus gained to move against the wind, without expending any sensible amount of its own energy. Here the upward motion is designedly arrested at any convenient stage —*e g.*, at each alternate pulsation of the wind—and the height attained is utilized, so that the action of gravity may carry the body by its descent in a curvilinear path (if necessary) against the wind. It has just been pointed out that if some height has been attained, the theoretical possibility of *some* advance against the wind in so falling hardly needs demonstration, though it may not unnaturally be supposed that the relative advance so gained must be insignificant compared with the distance travelled by the mean wind while the body was being elevated, so that on the whole the body is carried by the wind further than it advances against it.

This, however, probably need not be in fact the case, there being, as it appears to me from experiment and from deduction, every reason to believe that under suitable conditions the advance may be greater than the recession, or that the body, falling under the action of gravity along a suitable path, may return against the wind, not only from Z to O, the point of departure, but further, as is here shown.

I repeat, however, that I am not at the moment undertaking to demonstrate how the action is mechanically realizable in actual practice, but only that it is possible. It is for this purpose, and to understand more exactly that it can be effected, not only by the process indicated in the second illustration (fig. 2), but by another and probably more usual one (and nature has still others at command), that I have considered another treatment of the same conditions of wind pulsations always moving in the same horizontal direction, but for brief periods interrupted by equal intervals of calm. In this third illustration (fig. 3) we suppose the body to use the height gained in each pulsation to enable it to descend after each such pulsation, and advance against the direction of the wind.

The portion A B of the curve represents the path of the plane surface from a state of rest at A, where it has a small upward inclination toward the wind. If a horizontal wind blow upon it in the direction of the arrow, the first movement of the plane will not be in the direction of the wind, but, as is abundantly demonstrated by the writer in "Experiments in Aerodynamics," it will rise in a nearly vertical direction if the angle be small. The wind continuing to blow in the same direction, at the end of a certain time, the plane, which has risen (owing to its inertia and in spite of its weight) to the successive positions shown, is taking up more and more of the horizontal velocity of the wind, and consequently opposing less resistance to it, and therefore moving more and more laterally and rising less and less at every successive instant.

If the wind continued indefinitely the plane would ultimately take up its velocity, and finally, of course, fall when this inertia ceased to oppose resistance to the wind's advance.

I have supposed, however, the wind pulsation to cease at the end of a certain brief period, and, to fix our ideas, let us suppose this period to be five seconds. At this moment the period of calm begins, and now let the plane, which is supposed to have reached the point *B*, change its inclination about a horizontal axis to that shown in the diagram, falling at first nearly vertically with its edge on the line of its descent, so as to acquire speed, and this speed, acquired by constantly changing its angle, glide down the curve *B C*, so that the plane shall be tangential to it at every point of its descending advance. At the end of five seconds of calm it has reached the position *C*, near the lowest point of its descent, which there is no contra-

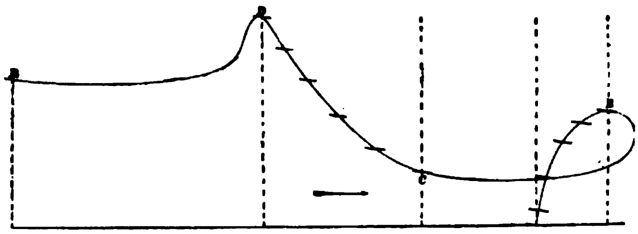


Fig. 3. A

diction to known mechanical laws in supposing *may* be higher than *A*, and which in fact, according to the most accurate data the writer can gather, *is* higher in the case of the above period, and in the case of such an actual plane as has been experimented upon by him.

Now, having reached *C* at the end of the five seconds' calm, if the wind blow in the same direction and velocity as before, it will again elevate the plane on the latter's presenting the proper angle, but this time under more favorable circumstances, for at this time the plane is already in motion in a direction opposed to that of the wind, and is already higher than it was in its original position *A*. Its course, therefore, will be nearly that along the curve *C D*, during all which time it maintains the original angle *a*, or one very slightly less. Arrived at *D*, and at the instant when the calm begins, it falls with varying inclination to the lowest position *E* (which may be higher than *C*), which it attains at the end of the five seconds of calm, then rises again (still nearly at the angle *a*) to a higher position, and so on, the alternations of directions of motion at the end of each pulsation growing less and less sharp, and the path finally taking the character of a sinuous curve. We have here assumed that the plane goes against the wind and rises at the same time, in order to illustrate that this is possible, though either alternative may be employed, and the plane, in theory at least, may maintain on the whole a rapid and nearly horizontal or a slow and nearly vertical course, or anything between.

It is not meant either that the alternations which would be observed in nature are as sharp as those here represented, which are intentionally exaggerated, while in all which has just preceded, by an equally intentional exaggeration of the normal action, the wind pulsations have been supposed to alternate with absolute calm. This being understood, it is scarcely necessary to point out that if the calm is not absolute, but if there are simply frequent successive winds or pulsations of wind of considerably differing velocity (such as the anemometer observations show are realized in nature) that the same general effect will obtain,* though we are not entitled to assume from any demonstration thus far given that the total advance will be necessarily greater than that of the whole distance the mean wind has traveled. It may also be observed that the actual actions of the soaring bird may be and doubtless are more complex in detail than those of this diagram, while yet in their entirety depending on the principles it sets forth.

The theoretical possibility at least will now, it is hoped, be granted, not only of the body's rising indefinitely or of its descending in the interval of calm to a higher level (C) than it rose from at A, but of its advancing against the calm or light wind through a distance, B C, greater than that of A B, and so on The writer, however, repeats that he has reason to suppose from the data obtained by him that this is not only a theoretical possibility, but a mechanical probability under the conditions stated, although he does not here offer a quantitative demonstration of the fact other than by pointing to the movements of the soaring bird and inviting their reconsideration in the light of the preceding statements.

The bird, by some tactile sensibility to the pressure and direction of the air, is able, in nautical phrase, to "see the wind" † and to time its movements, so that, without any reference to its height from the ground, it reaches the lowest portion of its descent near the end of the more rapid wind pulsation; but the writer believes that to cause these adaptive changes in an otherwise inert body, with what might almost be called instinctive readiness and rapidity, does not really demand intelligence or even instinct, but that the future aerodrome may be furnished with a substitute for instinct in what may perhaps allowably be called a mechanical brain, which yet need not, in his opinion, be intricate in its character. His

* The rotation of the body about a vertical axis so as to change the aspect of the inclination, as in the first figure, may be illustrated by the well-known habit of many soaring birds, of moving in small closed curves or spirals, but it may also be observed that, in view of the fact that even in intervals of relative calm, during which the body descends, there is always some wind; that in making the descents, if the body, animate or inanimate, maintain its direct advance, this wind tends to strike on the upper side of the plane or pinion. Mr. G. E. Curtis offers the suggestion that the soaring bird avoids such a position when possible, and therefore turns at right angles to or with the wind, and that this may be an additional reason for his well-known habit of moving in spirals.

† Mouillard.

reasons for this statement, which is not made lightly, must, however, be reserved for another time.

It is hardly necessary to point out that the nearly inert body in question may also be a human body, guided both by instinct and intelligence, and that there may thus be a sense in which human flight may be possible, although flight depending wholly upon the action of human muscles be forever impossible.

Let me resume the leading points of the present memoir in the statement that it has been shown :

1. That the wind is not even an approximately uniform moving mass of air, but consists of a succession of very brief pulsations of varying amplitude, and that, relatively to the mean movement of the wind, these are of varying direction.

2. That it is pointed out that hence there is a potentiality of "internal work" in the wind, and probably of a very great amount.

3. That it involves no contradiction of known principles to declare that an inclined plane, or suitably curved surface, heavier than the air, freely immersed in and moving with the velocity of the mean wind, can, if the wind pulsations here described are of sufficient amplitude and frequency, be sustained or even raised indefinitely without expenditure of internal energy, other than that which is involved in changing the aspect of its inclination at each pulsation.

4. That since (A) such a surface, having also power to change its inclination, *must* gain energy through falling during the slower and expend energy by rising during the higher velocities ; and that (B) since it has been shown that there is no contradiction of known mechanical laws in assuming that the surface *may* be sustained or *may* continue to rise indefinitely, the mechanical possibility of some advance against the direction of the wind follows immediately from this capacity of rising. It is further seen that it is at least possible that this advance against the wind may not only be attained relatively to the position of a body moving with the speed of the mean wind, but absolutely and with reference to a fixed point in space.

5. The statement is made that this is not only mechanically possible, but that in the writer's opinion it is realizable in practice.

Finally, these observations and deductions have, it seems to me, an important practical application, not only as regards a living creature, like the soaring bird, but still more as regards a mechanically constructed body, whose specific gravity may probably be many hundred or even many thousand times that of the atmosphere. We may suppose such a body to be supplied with fuel and engines, which would be indispensable to sustain it in a calm, and yet which we now see might be ordinarily left entirely inactive, so that the body could supposably remain in the air and even maintain its motion in any direction without expending its energy, except as regards the act of changing the inclination or aspect which it presents to the wind while the wind blew.

The final application of these principles to the art of aerodromics seems, then, to be, that while it is not likely that the perfected aerodrome will ever be able to dispense altogether with the ability to rely at intervals on some internal source of power, it will not be indispensable that this aerodrome of the future shall, in order to go any distance—even to circumnavigate the globe without alighting—need to carry a weight of fuel which would enable it to perform this journey under conditions analogous to those of a steamship, but that the fuel and weight need only be such as to enable it to take care of itself in exceptional moments of calm.

DISCUSSION OF PROFESSOR LANGLEY'S PAPER BY MR. CARL MYERS, FRANKFORT, N. Y.

Professor Langley's paper corroborates singularly well my observations of the undulatory movements of air, which I made several years ago, and then thought a discovery of my own.

If a sheet of paper or of flexible material be fastened by one edge and exposed to the wind so as to be upheld thereby, it at once flutters into a series of waves. It would seem that the wind should find easier passage by blowing the sheet into a straight line—by holding it stiff, as it were—but as a matter of fact it is not so; the passing air occasions the flapping which may be observed in the tail of a flag, particularly if it be hung to a horizontal instead of to a vertical line, and it alternately pushes the weight of the fabric up or down.

This movement can evidently be made a power in aerial navigation. I first discovered its possibilities when making some experiments in anemometry. I had connected with a flexible rod two surfaces, one rather large, with a small hollow, and another smaller. When the latter was presented to the wind the whole system was pushed back. When the larger aeroplane was presented to the wind, and the smaller one was behind, the latter had a movement up and down which seemed to urge the other forward, and its vibrations in the perpendicular direction resulted in a horizontal force, very much as the fish is propelled by the vibrations of his tail.

Subsequently I observed a similar action in some kites which I constructed with a flexible backbone, and which, when released, would advance relatively against the wind—that is, they would not drift back as fast as the wind blew. An account of some of these kite experiments will be found in the *Scientific American* supplement (No. 835) for January 2, 1892.

Basing myself upon these experiments, I have been holding for some years an undulatory theory of flight—it might be better said, an undulatory theory of air movements, which greatly resembles that of Professor Langley; but this is the first time, I think, that I have made the announcement in public.

There is no doubt in my mind as to the fact of the waviness

and intermittence of wind currents; and the question is how it can be applied in aerial navigation or in explaining the mystery of the soaring birds.

BY DE VOLSON WOOD, OF STEVENS INSTITUTE.

Professor Langley is a man of too high scientific attainments to set forth a theory in which he has not full faith and some foundation, so that I do not wish to speak of what he has done lightly; but I find it hard to concede that the variable velocity of the wind, as it moves forward past the point of observation, is a sufficient explanation of the soaring of birds.

This variable velocity—its "streakiness," as I prefer to call it—has long been known, although it has not been described in scientific literature. Mr. T. O. Perry, a former pupil of mine and a graduate of the University of Michigan, wrote to me in regard to experiments he had made on windmills, and stated that he had found great difficulty in obtaining reliable data because of the great "streakiness" of the wind; that when the anemometer indicated a considerable velocity of the wind, the windmill, only a few feet away from the anemometer, did not seem to feel the wind.

But perhaps we owe a great deal to Professor Langley for actually measuring this quality, although I cannot see the appropriateness of the term of "internal work," as applied thereto; and while the term "streakiness" is not euphonious, I believe it to be more expressive of the thing described.

In passing to the application, Professor Langley has alluded to Maxwell's "sorting demons." This theory, coming from so eminent a man as Maxwell, has carried a great deal of force as a hypothesis; but my opinion is that even this hypothesis is a failure, and that if Maxwell could get his demons to work just as he supposes them to do, he would find that they would not help him out in a wind. Professor Langley has done a very large amount of careful experimenting in air resistances, but no practical results are as yet apparent, and one is disappointed at the weakness of his arguments when he comes to the "application." These arguments amount to a begging of the question, to an appeal for the assent of the reader without sufficient demonstration, and not on a properly reasoned-out case. For instance, he says, "I think that, on considering these striking curves, we shall not find it difficult to admit,". . . And again, "The theoretical possibility, at least, will now, it is hoped, be granted." . . . Then certain hypotheses follow which are supposed to be supported by the fact that certain birds do soar!

From these experiments and reasonings the author reached the conclusion that it was theoretically possible for a heavy body, wholly immersed in the wind, "to move east while the wind was blowing west, without the use of any power other than that which the wind itself furnished, and this even by the use of plane surfaces." I fail, however, to comprehend

how the experiments and illustrations support the theory. The only certain fact is that certain birds do soar against the wind ; but the extent and the method remain to be determined and explained. I can only say that the conclusion of Professor Langley that the feat is "theoretically possible" is much more modest than the assertion of Mr. Lancaster, made some years ago, that he had made what he called paper "effigies," which, upon being exposed to the breeze, were picked up by the wind and floated off toward the east while the wind was blowing west. He utterly failed, however, to exhibit any such action of an "effigy" in the presence of any other person.

I have one more word : granting that the "streakiness" of the wind is a condition favorable—even a condition necessary—for soaring of birds, will not its effects be neutralized when applied to the large surfaces required to support an artificial machine for man ? for then the total effect will be practically the same as that of the "mean wind," which moves with nearly uniform velocity, so that if it be necessary to have this wavy motion of the wind up and down, right and left, and generally turbulent, to enable the small body of the bird to perform the soaring action, it by no means follows that the effect will not be lost, or nearly all lost, when applied to a larger area.

Upon the whole, there seems to be some principle not yet fully understood involved in the soaring of birds, and the possibility of reproducing it artificially does not yet appear to be within the grasp of man.

FROM PROFESSOR I. P. CHURCH, ITHACA, N. Y. (BY LETTER.)

Professor Langley's theory of soaring flight, in the paper called by him "The Internal Work of the Wind," is certainly a most reasonable hypothesis, fully in harmony with the known laws of mechanics, if we grant the postulate of a pulsating wind. This theory we find very carefully elaborated and illustrated in the paper in a qualitative way; though some quantitative arguments are not wanting, while an ample account is given of evidence showing the almost continual fluctuations of air velocity in an ordinary gale.

As to previous ideas in the same direction, Professor Langley quotes from Lord Rayleigh in a foot note on page 44 of the January AERONAUTICS ; while with regard to like opinions expressed later than the first presentation of the paper in question (April, 1893), the following references may be given : In the October AERONAUTICS (middle of first column, page 49), Mr. Winston presents in his "third method" a series of movements on the part of a soaring bird which, in the utilization of an *intermittent wind*, somewhat resemble the manœuvres suggested by Professor Langley in connection with fig. 3 of the latter's paper (page 50, January AERONAUTICS). Again, in his letter on page 336 of *Engineering News* of October 26, Mr. Wisner refers to the lack of uniformity in the velocity of the wind as an ample solution of the problem

of soaring. The same gentleman, in a private letter to the present writer, says, "As far as I have been able to determine, the same pulsations exist there (in a 'norther on the Gulf') as during gales farther north. It is a noteworthy fact that on all coasts with which I am familiar this variation of velocity is well shown by the waves action. The height of the waves is never uniform, but at regular intervals two or three large waves occur with smaller ones between." "Any one who has been at sea and listened to the ever-changing *note* of the wind whistling through the rigging cannot very well doubt that variation does exist."

As to the reasonableness of the theory that even in the upper air and in the region of the trade winds this fluctuation of the wind's velocity must or may exist, it may be well to consider the phenomena connected with the flow of water in an open channel. True, water is a liquid and not a gas; but from that very fact, the range of elastic movement being of so much greater amplitude in the case of the gaseous fluid, we should naturally expect that any pulsatory action observed in the flow of a liquid would have its counterpart in the behavior of moving gas, and also that this action in the latter case would be of a more pronounced character.

In Professor Unwin's article, "Hydromechanics," in the *Encyclopædia Britannica*, we read (page 461, second column): "*Periodic Unsteady Motion.*—In ordinary streams with rough boundaries it is observed that at any given point the velocity varies from moment to moment in magnitude and direction, but that the average velocity for a sensible period (say for five or ten minutes) varies very little either in magnitude or direction. It has, hence," etc.

We find evidence of the same general character in the fact, familiar to all experimenters with the flow of water in pipes, that the water column in the glass tube of an open piezometer inserted in the side of the pipe is rarely in a state of perfect rest, although a nominally "steady flow" is proceeding in the pipe. The observer is compelled to estimate an average position of the summit of the column on account of the oscillations.

In regard to the trade winds of the tropics, Professor Guyot says (in article "Winds," *Johnson's Encyclopædia*): "They blow with entire regularity only on the open sea;" and, further on, "Owing to this disturbing influence (continents), the trades, both in the Atlantic and Pacific, begin to blow regularly only at a considerable distance west of the continents."

It should also not be forgotten that the counter currents in the upper air—always a necessary concomitant of the trade winds (Guyot)—are probably a source of some vibratory disturbance to those winds—comparable, perhaps, to that of the continents underneath the latter.

On page 50 of his paper, Professor Langley refers to the "tactile sensibility" of the bird "to the pressure and direction of the air," as something that can be realized in the

"mechanical brain" of the future "aerodrome." By this we may suppose to be meant, perhaps, some mechanism involving a vane, a very light Robinson anemometer, and a pressure gauge, so connected (electrically) with the steering gear and motor (if motor there need be) as to bring them automatically into suitable action at the proper juncture—for example, to bring about a downward glide at the beginning of a comparative calm.

To gather evidence bearing on Professor Langley's theory it would seem desirable that some kind of self-registering apparatus should be carried in future "exploring balloons" to test and measure the pulsations of the wind at various heights. Of course the balloon would move with the "mean wind," to use Professor Langley's phrase, so that the apparatus would only record the difference between the momentary local air velocity and that of the mean wind ; but in this way the fact and extent of pulsations would be made apparent.

DISCUSSION BY J. B. JOHNSON, PROFESSOR CIVIL ENGINEERING, WASHINGTON UNIVERSITY, ST. LOUIS. (BY LETTER.)

In a search for the cause or causes of any natural phenomenon, it is but common prudence to examine the adequacy of known causes which may operate, before we begin to search for occult ones whose very existence has been hitherto unknown. Until known causes have been proved either inadequate or inoperative, we are not obliged to conclude that some hidden cause is at work. As to the soaring birds, the writer is not prepared to deny that the bird is sustained either by upward currents in the atmosphere, or by some sculling action of his tail, or by some invisible action of the individual feathers. However, the problem now in hand is rather to examine the adequacy of the explanation offered by the distinguished author of this paper, and to further scrutinize the foundation he has laid for the wonderful prophecy of aerial circumnavigation of the globe without alighting, so glowingly depicted in his closing paragraph.

Actual Wind Velocities.

The distinguished Secretary has given us some autographic records of very fluctuating velocities of the wind, and leaves us to infer that all winds have similar gusty variations. The writer is persuaded, from much observation of wind velocities across the top of a tall (150 ft.) chimney near his house, that the records given us by the author of the paper are not typical. He believes they are decidedy exceptional. Probably the lateral trend of a column of smoke rising vertically at a uniform velocity into the air is as good a measure of the relative instantaneous velocity of the wind as it is possible to obtain. The rising gas has so little inertia that it instantly takes the horizontal movement of the surrounding air. If some fixed coordinates, as a telegraph pole and its cross-arm, be brought into

line with the leeward side of the chimney, with the intersection at a point some distance above the top of the chimney, as shown in the accompanying sketch, then, since the upward motion is nearly constant for a short distance, the intercept $O\ A$ is a true relative measure of the horizontal velocity; that is, if the upper side of the smoke column fluctuates from A to A' and back to A, and if $O\ A' = 2\ O\ A$, then the velocity of the wind when the column falls to A is twice what it is when it rises to A'. This being, then, a very fair measure of the relative velocity of the wind (and one can form also some approximate notion of the absolute velocity), and since it responds instantly to all variations of velocity, it offers a ready and ever-present means of testing the universality of the wind variations shown in the autographic records given in the paper.

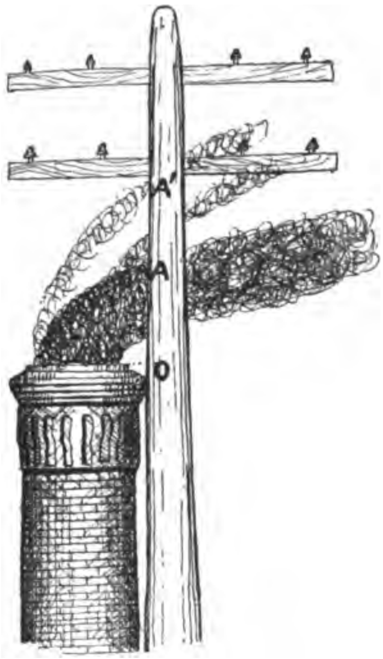


Fig. 1.

The writer has used this means of testing the gustiness of the ordinary atmospheric movements, and his conclusion is that the records shown in the paper are exceptional. But even though they be taken as typical, they do not prove to the writer anything like as much as they seem to have proved to the author of the paper. For instance, taking a generalized case of a wind varying every 10 seconds between 25 and 35 miles per hour as limits, or having an average of 30 miles per hour, it must be remembered that such winds are very rare. There were but 76 hours in the entire year 1892 when such winds were found in St. Louis, and but 97 hours in 1893. This is an average of about 1 per cent. of the time. These records were taken on the top of the United States Custom House, St. Louis, 150 ft. above the street and 250 ft. above low water in the Mississippi River, being 625 ft. above sea level.

At higher altitudes such velocities would of course obtain a greater portion of the time, but certainly they are rare, and probably do not obtain at any altitude more than from 5 to 10 per cent. of the time. They are, therefore, not to be relied on at all as a means of aerial transit; neither can they be assumed to explain the soaring of birds, since these are seen perhaps every day in the year in the lower latitudes.

The Soaring Bird.

Passing now to the assumed typical instance of an average 30-mile wind pulsating between 25 and 35-mile limits, it will be shown that the average sustaining velocity is only about *three miles per hour!* This can be seen from fig. 2, where the velocity is represented by the ordinates and the time in seconds by the abscissæ. The horizontal line represents the average speed of 30 miles per hour. The full curved line represents the pulsating velocity of the wind for the author's generalized case, where it passes alternate maximum and minimum points every 10 seconds, with an amplitude in the variation of 10 miles per hour.

In an elastic fluid like air the change from one extreme of velocity to its opposite would of necessity be by gradual variations of speed, as shown, so that this curve must be taken as fairly representing the author's typical case, the wind here varying from 25 to 35 miles per hour every 10 seconds.*

The dotted curved line represents the fluctuating velocity of the bird, which moves at the average velocity of 30 miles an hour with the wind. Whenever the wind is moving faster than the bird, the velocity of the latter is being accelerated, and *vice versâ*. The writer has assumed this fluctuating of the bird's velocity to be 2 miles faster and 2 miles slower, alternately, than its average velocity, or from 28 to 32 miles per hour. This variation in the bird's speed is only an assumption, but it would certainly be something. The author's own experiments show † that the horizontal resistance to a plane soaring at an angle of 7° (that selected in the paper) is 60 grammes per square foot of actual area of the soaring plane, or over one-fourth of its weight (in the case taken). If the horizontal resistance were only one-tenth of the weight, its acceleration and retardation would be one-tenth that of gravity, or 3.2 ft. per second, while the maximum rate of acceleration or retardation given in the assumed curve is only about 1 ft. per second.

Now the author has assumed, in his first case, that the bird is sailing or soaring with the wind at the same average velocity, and that its own inertia causes it to move at a nearly uni-

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* The author of the paper has assumed this change of speed to occur instantly, as shown in his fig. 2, p. 49, AERONAUTICS. This is, of course, absurd.

† "Experiments in Aerodynamics," by S. P. Langley, "Smithsonian Contributions," No. 801, p. 64.

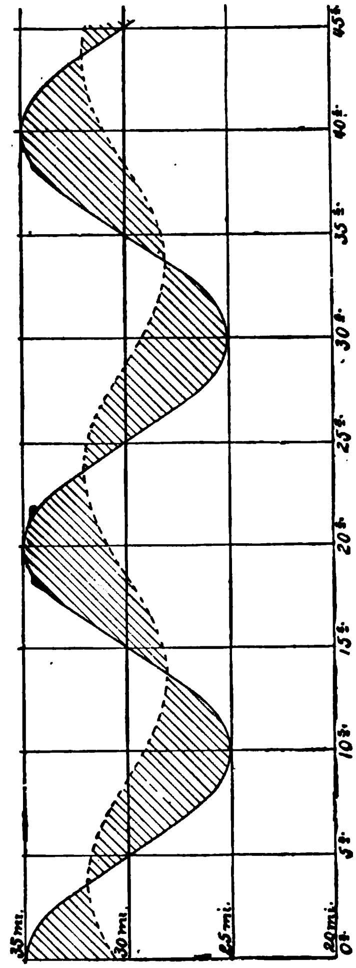

Fig. 2.

form velocity while the wind fluctuates, as shown in fig. 2. The wind, therefore, moves alternately faster and slower than the bird, and the difference between the velocities of the wind and of the bird is taken advantage of by the latter by tilting his wings up behind when the wind is faster and up in front when the wind is slower (although there would be no reason,

of course, why the bird should not face the other way and soar backward just as well)

It is important now to examine this case and see just what these alternate sustaining velocities would be. These relative velocities, on which the bird alone relies to sustain him, are shown in fig. 2 by the shaded areas. So long as this area lies above the dotted line, the bird's velocity is being accelerated, and *vice versâ*; hence the curve marking the wind velocities crosses the one marking the bird's velocity at the maximum and minimum points of the latter, as shown, and since the pulsations are at uniform intervals (in the generalized case) both curves are symmetrical about the line representing the mean velocity.

An examination of this diagram shows that *the sustaining relative velocity varies from nothing to five miles per hour, with an average value of about three miles per hour.* Would such a relative velocity sustain a soaring bird? Manifestly not. It would not sustain the lightest paper kite. The author of the paper assumes a case where a velocity of 36 ft. per second, or 25 miles per hour, will sustain and slowly lift a plane weighing 0.43 lbs. per square foot. If the bird weighs 10 lbs. it would require 23 sq. ft. of surface elevated to an angle of 7° and held against a relative wind velocity of some 20 miles per hour to sustain it from falling. Does the bird have anything like this superficial area of sustaining surface? Evidently not. With a smaller superficial area it would require a higher velocity. What relative velocity it would require to sustain a buzzard the writer does not undertake to say, but he feels rather confident that it would have to be somewhat greater than 3 miles per hour!

The author's second case assumes that the bird not only sustains himself by means of this relative 3-miles velocity, but that by circling about and by some kind of rising and falling action he not only sustains himself, but that he also maintains his mean geographical position. To do this requires a much greater "internal work of the wind" than simply to overcome the attraction of gravity, for the frictional and cross-sectional area resistance of the air must also be counteracted. If, therefore, the author of the paper has not convinced us of the truth or possibility of his first case, much less can we pin faith to his second.

Conclusions.

In conclusion, the writer's objections to the legitimacy of the conclusions of the author are:

(*a*) That the extreme fluctuations of wind velocities shown are thought to be exceptional and not typical.

(*b*) Even if they were typical, such high velocities are extremely rare, and could not explain the ever soaring of the birds.

(*c*) But even though they were continuous they offer but an insignificant relative velocity and entirely inadequate for sus-

taining a bird, this relative velocity being but about 3 or 4 miles per hour in the case selected by the author.

(*d*) That if the theory advanced is inadequate to explain the soaring of a bird when advancing with the wind at its average speed, much more is it inadequate to explain the soaring of a bird in a high wind when he maintains his mean geographical position.

(*e*) That the great wonder is, perhaps, not so much that scientists should have neglected to adequately study this problem as that one of the greatest American scientists should have gravely advanced such a hypothesis with such absolute confidence to explain the phenomenon.

FROM H. A. HAZEN.

I would like to give a leaf or two of my experience in connection with this most interesting paper of Professor Langley's, printed in January AERONAUTICS. I am inclined to think that most of the sudden fluctuations in wind velocity described in this paper were due to a sort of heaping up of the air against obstructions, and then a sudden giving way of the air under abnormal pressure or to a peculiar effect of the wind on the backs of the light cups. If we try experiments on the resistance of still air against plates moved very rapidly through it, we will find that the plate at first continually oscillates back and forth, owing to a heaping up of the still air in its front. It would seem that some such effect as this would take place on a much larger scale from the side of a building or tower. In several ascensions in balloons I have suspended, from the basket by cords 20 to 30 ft. long, a lead weight and a small toy balloon (10 in. diameter, inflated with coal gas), and have always found an angle between them—that is, sometimes the small balloon would be ahead and sometimes the weight. These changes were due to a change in the velocity of the current, and took place quite slowly. The small balloon was an extremely delicate indicator of any change in the current. When the large balloon began to go down the small balloon went up into the air, clear to the end of the string, and floated there as long as there was a descent in the large balloon. A statement of these experiments will be found in the *American Meteorological Journal*, November, 1891, page 293. I had suggested some such influence as this upon a soaring bird, in an article written two months ago, shortly to be published, and not knowing of this previous paper of Professor Langley's. It seems to me this effect in the upper air must be extremely slight, and can account for only a small part of the energy needed to keep the bird afloat.

This is not all. I understand that Mr. Lancaster closely observed soaring birds in Florida not many years ago, and found them soaring under all conditions, and oftentimes in a dead calm. If this be granted, all explanations of this kind or any invoking of the aid of the wind must fail. I do not say there might not have been a slight wind at the bird not felt

on the tree-top where the observer was, but he strongly maintains that there was no appreciable wind, and oftentimes the birds struck the mask he had over him in the tree-top.

FROM C. F. MARVIN, UNITED STATES WEATHER BUREAU.

The explanation of the soaring of birds, as developed by Professor Langley in his paper upon the "Internal Work of the Wind," presents many remarkable features, and the energy derivable by his theory from the great and ever-changing velocities which characterize the ordinary movement of the atmosphere known as wind, may be sufficient, quantitatively, to account for the wonderfully graceful act in question, especially when the soaring object is small and able to take advantage of conditions which it seems must be highly localized. In the case of bodies of considerable extent of surface, it would seem the energy available must be seriously diminished by the opposition of contending effects at different portions of the soaring surfaces.

Having in mind the various modern explanations of this perplexing phenomenon, I have utilized every opportunity to scrutinize the soaring of birds, and remember being impressed with the seeming inadequacy of all solutions thus far proposed to account for an instance of soaring I watched at the summit of Pike's Peak in 1892. The bird was similar in appearance to the turkey buzzard, but I believe of somewhat smaller size. Its movements were under my view for only a minute or two, but during this time it sailed gracefully around in comparatively close proximity to the precipitous faces of the mountain summit, which at this place formed a partially enclosed and sheltered well of large proportions and very rugged, almost perpendicular walls. The bird was, in general, not more than 100 to 200 ft. below the level of my own position. The wind movement at the time of my observation was very slight, and was believed to be greatly lessened in the vicinity of the bird by reason of the sheltering action of the mountain walls. Considering how greatly the density of the air was diminished at this elevated point, 14,000 ft., and its almost stagnant condition, it seemed very difficult to realize that the energy required in the movements I saw could be derived by virtue of differential motions of the atmosphere itself. The new factors brought out by Professor Langley's theory do not appear to remove the difficulties.

Every observant meteorologist, as well as many others, we believe, have always realized, even without instrumental investigation, that the current of the wind is exceedingly tumultuous and characterized by just such extreme variations of velocity as those presented in Professor Langley's diagrams. During the winter of 1888–89 the writer was engaged upon a series of experimental investigations upon the measurement of wind velocities by means of the Robinson anemometer. In the course of these it was found that the extreme and very

rapid changes in velocity of all ordinary winds caused a very noticeable effect upon the rate of rotation of the ordinary Robinson anemometer, whereby heavy cups were made to indicate a somewhat higher velocity than light cups. To investigate this effect more fully and to show the extremely variable character of the wind velocity, comparisons were made between very heavy, medium, and very light paper cups, and, in addition, a small anemometer, about one-quarter size, with exceedingly light, cone-shaped cups, weighing only 2¼ grams, was employed.

The electrical registration of these instruments was made generally for each 25 revolutions of the cups, which happens to be the same as in the case of Professor Langley's first experiments at Allegheny. Two of the anemometers used by the writer, however, were made to record with each revolution of the cups, and the character of the wind as regards its sudden extreme variability of velocity was fully established.

Some of the results of these studies, showing the effect of this great variability upon different anemometers, was published in the *American Meteorological Journal* for April, 1889, pp. 552, *et seq.*

Learning of Professor Langley's desire to study the variability of wind velocity, it was with great pleasure the writer placed at his disposal, through the Chief of the Weather Bureau, the identical anemometer, provided with electrical registration for each revolution and with the light paper cups used in his own experiments in 1889.

Still further experimental evidence of the extreme variability of the velocity of the wind was presented to me while making a series of experiments to directly measure the pressure of the wind upon normally exposed plates. These experiments were made during August, 1890, at the summit of Mount Washington, where the wind velocities registered from very light to about 60 miles per hour. The pressure was graphically and automatically recorded upon a sheet of moving paper. Quoting from the published account of these experiments in the *Engineering News* for December 18, 1890, p. 521, it is stated, in reference to the trace derived automatically from the pressure plate, that, "The curve of pressures, if it can be called a curve, presents, in spite of the comparatively rapid rate of rotation of the register, a very irregular appearance indeed. The oscillations do not, except for occasional instants, correspond to harmonic vibrations of the spring and pressure plate considered as a vibratory system, but are actual and real changes in wind pressures. The magnitude of these variations is, itself, very irregular, but it may be stated to be approximately 35 per cent. of the mean pressure. There is in addition to these very rapid variations in the pressure, which take place inside of a second or two of time, other variations which go through their irregular changes in from a few to several minutes' time;" and further on: "In estimating the strains to which engineering structures may be subjected by winds . . . it is important to note that momentary pressures

as much as 85 per cent. in excess of the mean pressure may continually occur and recur."

These characteristics of ordinary wind movement must have been almost self-evident to many, but few probably have sought to discover in them any useful application.

C. F. MARVIN,
Professor in charge of Instruments,
United States Weather Bureau.

WASHINGTON, D. C., January 9.

FROM GEORGE CROSLAND TAYLOR.

I have read the above paper with great interest. It has not occurred to me before to doubt that winds at high elevations above ground could not but be more or less steady in velocity, and certainly not to the extent the diagrams show.

I have arrived at somewhat similar general conclusions for wind currents near the ground on exposed situations on hills from 200 to 460 ft. above sea-level near the coast. In 1890 I invented and patented a substitute for string æolian harps having free reeds in lieu of long strings; these reeds are mounted on concave or V-shaped surfaces; the reeds vibrate in the wind, and are tuned in harmony. According to quality, the reeds last from two to three years' exposure.

The best for observations on force of the wind are mounted like wind vanes, with a tail attached to keep the head on to the winds, on a wire spike passed through the box and driven into a stick some 8 ft. above ground level. By listening to several of these in various positions, I have long formed the conclusion that such a thing as a steady wind does not exist at all at any height up to 35 ft. from ground, which is as high as I have tried them, even when it feels to blow steady.

The sounds, which can be distinctly heard, vary in intensity with the force of the wind—sometimes silent, at others a faint hum, perhaps from one reed only out of 40, to a full blast, even in what might be called a moderate, steady gale, which immediately starts the three or four octaves the reeds may be tuned to, wherein consists the charm of an æolian harp. But until I read Professor Langley's paper it had not occurred to me to take particular notice of this effect as important to be taken account of, as is pointed out in the paper, in regard to aeronautical experiments, except I have regarded it as a rough guide for the time being as to the velocity of the wind. The sounds vary much according to the weather, and it is not at all difficult to predict with tolerable correctness, from the timber of the note tones, either rain, moist winds, or cold, several hours in advance. Coming rain is indicated by a weird, undulating flow of sound—by preference notes in the minor key are vibrated; if for dry, cold weather, the tone is shrill, better sustained, and the major notes come into play more.

The vane or tail of the instrument veers from side to side a few degrees at times; this may mean change in direction of current sideways, and where the contour of the ground is uneven and hilly, there is no doubt upward and downward

movements for perhaps 1,000 ft. or more vertically. I have observed also that the winged planes flown from a pole, as described in my paper to the Chicago Aeronautical Congress last August, hardly ever remain fixed, but have a tendency to constantly hover about, up and down, and sideways

I am pleased to see that men of such undoubted ability as Professor Langley and others are in America seriously taking up the study of aerial flight, and I agree with Professor Langley that it is highly probable that the flight of birds is not the result of great muscular exertion on their part, but a result derived from the facility with which they take advantage of the varied currents and velocities of the wind.*

ANEMOMETRY.

BY S. P. FERGUSSON.

AN accurate knowledge of the behavior of air currents being essential to the study of aerial navigation, it is obvious that the instruments used for measuring these currents should be of the greatest accuracy. The object of this paper will be to give, as far as possible, an account of recent progress in the special department of anemometry, and descriptions of the best apparatus for general and special use.

A perfect instrument for measuring either the direction or velocity of the wind would of necessity be without weight and frictionless in action; such an instrument being impossible, however, the object of experiment should be to evolve an instrument that will most nearly conform to these requisites. Continuous observation has shown the wind to be extremely variable, both in direction and velocity, and that large oscillations in either often occur within a few seconds of time; also that the variability increases as the velocity rises. At Blue Hill Observatory extreme gusts have ranged as high as 110 miles an hour, while the mean velocity for an hour has never exceeded 78 miles; hence, from these observations it follows that the anemometer for general use should be light enough to indicate the rapid oscillations of the wind, and at the same time be strong enough to withstand the high velocities to which it may be exposed. The requirements of lightness and strength having been fairly met, the next step is to determine the relation between the indications of the anemometer and the movements of the wind. The lack of suitable means of determining this relation forms the chief obstacle to the production of a good anemometer. The most approved method for calibrating a velocity anemometer is to move it through still air at a known rate and compare the indications of the instrument with this rate; the apparatus usually employed being a long arm of known length rotating about a vertical axis, the anemometer to be tested being secured to the outer extremity of this arm. In practice, however, it is almost impossible to reproduce in this way the conditions prevailing

* See Appendix B. p. 402.

when the anemometer is exposed to the action of the natural wind, as it is difficult to impart a variable motion to the whirling arm, and also the behavior of some anemometers is very different in steady winds to what it is in variable winds. Also, if the calibration of the instrument is done in a closed room, as frequently happens, the continued rotation of the whirling arm soon sets up a rotation in the air surrounding it, the effect of which it is difficult to make proper allowance for. This difficulty is not encountered when the tests are made in the open air, but in this case the effect of the natural wind seriously impairs the accuracy of the work, unless it is done on calm days; and calm days are seldom obtained anywhere. Another source of error is the effect of centrifugal force upon the bearings of the anemometer when the speed of the whirler is at all great, which causes more or less friction; but this may not be a matter of such importance as the first two named.

Of recent whirling machine experiments, those conducted by Marvin in this country, and by Whipple and Dines in England, are among the most noteworthy. Marvin's experiments were conducted in a large enclosed court or room, and consisted chiefly of determinations of the constants of the Weather Bureau type of Robinson anemometer. A detailed account may be found in the *American Meteorological Journal* for April, 1889. The experiments made by Whipple and Dines were very extensive, and included tests of large and small Robinson anemometers, "windmill" anemometers and pressure plates, and measurements of the wind pressure upon various surfaces. The results from these experiments were published at intervals in the *Quarterly Journal of the Royal Meteorological Society* during 1889, 1890, 1891, and 1892. A direct comparison of anemometers tested by Dines and Marvin was begun at Blue Hill Observatory in 1892, a preliminary account of which appeared in the *American Meteorological Journal* for January, 1893. This work has not been completed, but the results may be published during the coming winter.

DIRECTION INSTRUMENTS.

Of instruments for measuring the direction of the wind there are two types in general use the well-known arrow vane or weathercock, and the windmill vane, which is used to some extent in Europe.

For scientific purposes the arrow vane is made with its tail composed of two plates usually diverging about 22°, though Curtis has found that for maximum sensitiveness the half angle of the two plates should be the average angle through which the wind makes sudden variations, the average being taken from 0° to 30°. A vane made of two very thin sheets of aluminium secured by light wires to a central rod of brass tubing at an angle of about 15°, and used experimentally at Blue Hill Observatory, has been found very sensitive and to meet all the requirements of a vane for indicating the direction of both variable and steady winds.

Whether the vane should be long or short depends upon the use to be made of it. A long vane tends to indicate the mean direction, as by it the smaller and more rapid oscillations are smoothed out; and while such an instrument is to be preferred when used only for the registration of wind directions from day to day, there are instances where a short vane will give the best results. These are when the vane is used to measure the short oscillations from second to second, or to support a velocity anemometer of the windmill type. At Blue Hill it was found that the same windmill anemometer, when mounted upon a long vane, continually gave lower velocities than when mounted upon a vane of the same construction but only half as long. Experiments to determine the length of vane best suited to windmill anemometers would be valuable, as many of these instruments are now in use, and until some uniform standard vane is adopted, the indications of widely separated instruments will not be strictly comparable.

The arrow vane has been used for measuring the vertical variations of the wind, and in fair weather it is generally satisfactory; but for continual use it is not trustworthy, as ice, rain, or even dew collecting upon its tail disturbs the balance seriously, causing it to indicate descending currents when none may exist. For this reason, Dechevrens, who used this form of vane at Zi-ka-wei, China, in 1881, abandoned it as too unsatisfactory for continuous use.

One of the best types of the wind-mill vane is that forming the direction indicator of the Kew pattern or Beckley anemograph, which has been extensively used in England since 1858. This instrument consists of two fan wheels secured to opposite ends of a horizontal spindle which revolves in a frame rotating about a vertical axis. A worm or screw upon the spindle works in a large gear wheel fixed to the outside tubular support of the instrument through which the axis of the vane passes. In operation, when the direction of the wind changes, the wheels are set in motion and revolve until the screw brings their spindle at right angles to the wind, in which position the wind cannot turn them until the direction again changes. The accuracy with which this instrument follows the variations of the wind depends upon the speed of the wheels in adjusting themselves; and as several rotations may be necessary to effect adjustment during a wide oscillation, it is obvious that such an instrument will indicate more nearly a mean direction than the actual oscillations of the wind. This form of vane occupies much less space than an arrow vane, but for indicating rapid changes or aligning a windmill anemometer, it is not so satisfactory. Its complicated structure also renders it more liable to damage.

PRESSURE AND VELOCITY INSTRUMENTS.

There are two classes of velocity anemometers—pressure anemometers, which measure the force of the wind upon surfaces of known area, and rotation anemometers, which meas-

ure the velocity of the wind by the rotation of a series of windmill sails or cups. Of the pressure instruments, one of the most satisfactory forms is that of a plate of known area (usually 1 sq. ft.), which is kept facing the wind by a vane. The rod carrying the plate extends through the vane and carries a pencil or other indicator, which registers its motions upon a chronograph placed in the tail of the vane. An instrument of this type used at Blue Hill for several years gave satisfactory records for such an instrument, and was usually in close agreement with the velocity anemometers. The records of pressure anemometers are not so trustworthy as those of rotation anemometers, especially at low velocities, as the pressures are then very small; in most cases the instrument ceases to work below 10 miles an hour. As in case of the windmill anemometer, the indications of the pressure plate depend more or less upon the character of the vane used to keep it normal to the wind, which, for the best results, should be light, of moderate length, and with the plates composing it rather widely separated in order to avoid as much as possible eddies caused by the pressure plate itself.

Investigation has shown that pressure instruments of different sizes do not register alike, the smaller plates registering more than the larger; hence it follows that accurate comparisons can be made only when the instruments are similar. A curious fact, noted by Dines, is that holes bored in a pressure plate do not materially alter its readings within certain limits. A square plate 1 ft. in area was pierced with eight holes 1 sq. in. in area each, their total area amounting to over 5 per cent. of the plate; but the pressure was not perceptibly diminished, although a difference of 1 per cent. would have been noticeable. Details of these experiments were published in the *Quarterly Journal of the Royal Meteorological Society*, October, 1889. As of possible interest in connection with the subject of wind pressure, the following table of observed pressures on various surfaces, determined experimentally by Mr. Dines, is copied from the publication referred to above.

The table on following page shows the pressures at a velocity of 20.86 miles an hour. The values are reduced to the standard temperature and pressure. The flat plates were cut out of hard wood ⅜ in. thick, and allowance has been made for the arm which carried them.

Although much attention has been given the subject, there is yet more or less uncertainty as to the relation of the pressure to the velocity of the wind, there being several different formulæ for the conversion of one into the other. Until more accurate methods of calibrating both pressure and velocity anemometers are discovered, this uncertainty is likely to remain. The wind pressure has been found to vary approximately with the square of the velocity, and the formula generally used for reducing wind pressure to velocity is

$$p = 0.005\, v^2,$$

in which p = the pressure in pounds per square foot of sur-

PLATES TESTED.	Actual Pressure in Lbs.	Pressure in Lbs. per Sq. Ft.	No. of Experiments.
A square, each side 4 inches	.17	1.51	4
A circle, 4.51 inches diameter, same area	.17	1.51	9
A rectangle, 16 × 1 inches	.19	1.70	7
A circle, 6 inches in diameter	.29	1.47	7
A rectangle, 16 × 4 inches	.70	1.58	4
A square, each side 8 inches	.66	1.48	8
A circle, 9.03 inches in diameter, same area	.67	1.50	12
A square, each side 12 inches	1.57	1.57	7
A circle, 13.54 inches diameter, same area	1.55	1.55	14
A rectangle, 24 × 6 inches	1.56	1.59	6
A square, each side 16 inches	2.70	1.52	6
A plate, 6 inches diameter, 4¾ inches thick	.28	1.45	5
A cylinder, 6 inches in diameter, 4¾ inches long	.18	0.92	4
A sphere, 6 inches in diameter	.13	0.67	8
A plate, 6 inches in diameter, with a blunt cone, angle 90° at the back	.29	1.49	4
The same, with the cone in front	.19	0.98	4
A plate, 6 inches diameter with a sharp cone, angle 30° at the back	.30	1.54	4
The same, with cone in front	.12	0.60	4
A 5-inch Robinson cup, mounted on 8⅝ inches of ⅜ inch rod	.28	1.68	8
The same, with its back to the wind	.12	0.73	4
A 9-inch cup, mounted on 6⅜ inches of ⅜-inch rod	.82	1.75	8
The same, with its back to the wind	.28	0.60	8
A 2¼ inch cup, mounted on 9¾ inches of ⅜ inch rod	.13	2.60	3
The same, with its back to the wind	.05	1.04	3
One foot of ⅜ inch circular rod	.09	1.71	9

face exposed normally to the wind, and v = the velocity of the wind in miles an hour. This formula takes no account of the temperature or density of the air or for the factor of the anemometer used, which, in case of the Robinson anemometer, would be a matter of great importance. For these reasons this formula can only be regarded as an approximation. The following formula, proposed by Ferrel as the true theoretical formula, admits of a correction being made for barometric pressure and air temperature :

$$p = \frac{0.002698\, v^2}{1 + .004\, t} \frac{P}{P_0},$$

in which P_0 is the standard barometric pressure, 760 mm., P is the pressure at the station of observation, and t is the temperature on the centigrade scale. By theoretical formula is meant the formula which would hold in case of no viscosity of the air. For an average temperature, say 15° centigrade, and air of the standard pressure, 760 mm., this formula becomes :

$$p = 0.00255\, v^2.$$

(For discussion of various formulæ, see *American Meteorological Journal*, August, 1887.) Mr. Dines gives as the result of numerous experiments the formula

$$p = .0029\ v^2,$$

assuming, however, that the factor of the Robinson anemometer he used is 2.00 instead of 3.00, which is generally used, while Professor Marvin proposes the following:

$$p = .0040 \frac{B}{30} S\ V^2,$$

in which $S =$ the surface in square feet, $V =$ corrected wind velocity, and $B =$ the height of the barometer in inches. By corrected wind velocity is meant the velocity given by the Weather Bureau anemometer, corrected by the formula determined by Professor Marvin (*American Meteorological Journal*, February, 1891).

The experiments upon which the above formulæ are based were made mostly at low velocities, excepting, perhaps, those made by Mr. Dines, and much dependence cannot be placed upon them at velocities higher than 50 miles an hour. As extreme pressures are often much higher than that corresponding to this velocity, experiments made at higher velocities will be of great value in determining the efficiency of these formulæ at high velocities.

Of rotation anemometers, two types are in general use—the windmill anemometer, which is made in the form of a small windmill kept normal to the wind by a vane, and the Robinson cup anemometer, which consists of four hemispherical cups facing the same way around a vertical axis, and is, therefore, independent of changes in the direction of the wind. Of the two types, the windmill anemometer is much the older, and many different forms are now in use, principally for measuring currents in ventilators and in mines, for which it is well adapted. For meteorological purposes one of the best forms in use is that known in connection with its recording apparatus as Richard's anemo-cinemograph, which was placed on the market in 1888 by Richard Brothers, of Paris. The fan-wheel used as a motor is made of six blades of aluminium inclined at 45° and fastened to a very light axis. The diameter of this wheel is so calculated that one revolution corresponds to a meter of wind, this ratio being verified upon a whirling machine. The recording apparatus differs from all others in use in that it performs mechanically the operation of the division of space traversed by the time occupied, the pen tracing a curve corresponding either to the direct velocity per second or the mean velocity, as may be desired. In one case a contact is made for each meter of wind or each revolution of the fan wheel, and to properly register it the speed of the paper should be at least 3 mm. a minute. In another type the contacts are made for each 25 meters, and the mean velocity only is registered upon a drum revolving once daily. One of these ane-

mometers, used at Blue Hill for several years, has proved to be a very good instrument. The vane supplied by the manufacturers should be replaced by a spread-tail vane about 6 in. longer, to obtain the best results, however, as the friction of the electrical contacts prevents the vane from turning readily into the wind at very low velocities. At Blue Hill there has been in use a form of contact in which electrical connection between the vane and its support is made by steel points attached to the vane, dipping into circular troughs of mercury placed around the support. There is no increased friction when this device is used, but the steel points became rusted or coated with oxide so frequently that it was not considered a practical success, though for occasional use and in dry climates it would without doubt be excellent. The cinemograph used at Blue Hill agrees closely with the Weather Bureau anemometer (factor 3), and therefore registers about 20 per cent. too high. Its rate appears to be constant, however. The fan-wheel is very light, weighing only 150 grammes (about 6 oz.), and is very suitable for registering variable winds.

A type of fan anemometer having four blades is used for measuring the vertical component of the wind, its axis being placed vertical, and while in this position it can only be moved by ascending or descending winds. This instrument was invented about 1885 by Dechevrens, who designated it as a "clino-anemometer," and used it at the Zi-ka-wei Observatory, in China, for several years. As manufactured by Richard, the instrument consists of the fan-wheel and a recording apparatus designed by Garrigou-Lagrange, of Paris. Three electrical contacts are made at each revolution of the wheel, which turns once for each two meters of wind. The currents from these, transmitted to the register, operate successively three electro-magnets arranged symmetrically around a crown wheel having four teeth, into which play the armatures of the magnets, which turn it in the same direction as the fan-wheel. This crown wheel gives motion to the record cylinder, which accordingly rotates in one direction for a descending and in the opposite for an ascending wind, a pen moved by clock-work recording its motions. An instrument of this type was used regularly at Blue Hill for eight months, and one has recently been installed at Washington by the U. S. Weather Bureau. A discussion of the Blue Hill observations can be found in the *Annals of Harvard College Observatory, Vol. XL, Part I, Appendix B.* The proper exposure of this instrument is a matter of great importance, as unless its axis is placed exactly vertical, and the instrument at such a height as to be out of reach of disturbances caused by trees, hills, or buildings, etc., the records will not be of great value. A high skeleton tower erected on a level plain affords the best exposure.

Another type of windmill anemometer is that known as the helicoid, invented in 1887 by Dines, of which the motor is a single blade of very thin aluminium, resembling a ship's propeller. The instrument has been placed on the market as an "air meter," and indicates by dials the velocity of the air in

feet. The inventor has made many experiments with different sizes of this instrument, and concludes that its rate is constant and independent of size or velocity, and that the velocities indicated by it are nearer correct than those given by other anemometers. The "wheel" of the meter only weighs a fraction of an ounce, although about 6 in. in diameter, and there is good reason for considering it one of the most sensitive velocity anemometers ever invented. The Blue Hill comparison of this air meter with the Weather Bureau anemometer indicates that it registers about 10 per cent. less than that instrument.

The Robinson cup anemometer, designed in 1846, has been adopted as a standard by the principal weather services of the world, including that of the United States. Many different patterns and sizes have been constructed and are now in use, but the following are the principal forms used :

TYPE OF INSTRUMENT.	Diameter of Cups, Inches.	Length of Arms, Inches.	Where Used.
Robinson, first model..	13.0	23.0	England, Ireland.
Beckley or Kew Pattern	9.0	24.0	Standard in England and Colonies.
Deutsche Seewarte. ...	5.0	9.5	Germany.
U. S. Weather Bureau..	4.0	6.7	U. S. Standard since 1870.

The cups were originally supposed to revolve with one-third of the wind's velocity irrespective of size or distance from their center of rotation, but recent experiments have determined that neither the ratio $\frac{1}{3}$ nor any other will apply to all sizes of the instrument. As a rule the larger instruments register higher than the smaller. Instruments the cups of which are large compared with the length of their arms register higher at low velocities and lower at high velocities than do those with relatively smaller cups and longer arms, the reason being that the cups of the compact anemometers shelter each other at certain points during their rotation. The factor varies also according to the wind, the cups moving faster in a variable wind than in a steady wind. The factor of the Kew pattern was found by Dines to be 2.10, and practically constant. That of the U. S. Weather Bureau pattern was found by Marvin to be variable, ranging from about 3.10 at 10 miles an hour or less to about 2.30 at velocities of over 50 miles, the average being about 2.50. The comparisons of instruments tested by Dines and Marvin at Blue Hill included a Kew pattern and two Weather Bureau Robinson anemometers, a helicoid air meter and a cinemograph. The records have not all been reduced, but the results already obtained appear to show that the results obtained by Dines and Marvin do not agree quite as closely as accuracy demands.

The following are the differences, in per cent., of the Kew Weather Bureau and cinemograph from the helicoid:

Kew Pattern.		Weather Bureau Pattern.		Cinemograph.
Factor 3.00.	Factor 2.10.	Factor 3.00.	Factor 2.50.	
+ 20 per cent.	− 10 per cent.	+ 10 per cent.	− 6 per cent.	+ 10 per cent.

The helicoid used by me was tested by Mr. Dines, who found its error to be less than 1 per cent.; but assuming that Mr. Dines's factor 2.10 for the Kew pattern is correct, there yet remains a difference of about 10 per cent. between instruments of the types calibrated by Mr. Dines. Comparing Marvin's results with Dines's, there appears to be an average difference of about 4 per cent. between them. The relation of the Kew pattern and the Weather Bureau anemometers appears to be constant at velocities ranging between 10 and 35 miles an hour, as the variation of individual readings from the average was small. These conclusions, however, are based upon averages of a few observations at intervals of about 10 miles, for velocities ranging from 10 to 35 miles an hour, and may be modified somewhat when more data have been collected. Comparisons at higher velocities have been made, but the records have not yet been reduced.

As soon as possible the comparisons at Blue Hill will be completed and the results published; and it is hoped that this work will at least aid in rendering more accurate the comparison of widely separated wind records.

APPENDIX.

Papers on anemometry consulted in the preparation of this paper:

On Large and Small Anemometers, by Rev. F. W. Stow, *Quarterly Journal of the Royal Meteorological Society, Vol. I,* p. 41.

On the Determination of the Constants of the Cup Anemometer by Experiments with the Whirling Machine, by Rev. T. R. Robinson, *Philosophical Transactions of the Royal Society,* 1878–80.

Sur l'Inclinaison des Vents, by Rev. Fr. Mark Dechevrens.

Treatise on Meteorological Apparatus and Methods, by C. Abbe, *Annual Report of the Chief Signal Officer*, 1887, *Appendix* 46.

Recent Advances in Meteorology, by W. Ferrel, *Annual Report of the Chief Signal Officer*, 1885, *Part II.*

Measurement of Wind Velocity, by C. F. Marvin, *American Meteorological Journal,* April, 1889.

Anemometer Studies, by C. F. Marvin, *American Meteorological Journal*, July, 1889.

Wind Pressures and the Measurement of Wind Velocity, by C. F. Marvin, *American Meteorological Journal*, February, 1891.

On the Wind Pressure on Curved Vanes, by W. H. Dines, B.A., *Proceedings of the Royal Society*, *Vol. L.*

Account of Some Experiments made to Investigate the Connection between the Pressure and Velocity of the Wind, by W. H. Dines, B.A., *Quarterly Journal of the Royal Meteorological Society*, October, 1889.

Report of the Wind Force Committee on the Factor of the Kew Pattern Anemometer, *Quarterly Journal of the Royal Meteorological Society*, January, 1890.

On Wind Pressure upon an Inclined Surface, by W. H. Dines, *Proceedings of the Royal Society*, *Vol. XLVIII.*

Anemometer Comparisons, by W. H. Dines, *Quarterly Journal of the Royal Meteorological Society*, July, 1892.

THE AIR PROPELLER.

By H. C. Vogt, Naval Experimenter, Copenhagen, Denmark.

As most of the experiments performed with the air propeller were brought before the British Association in September, 1888, and published in *The Engineer* of September 28, 1888, and in *Industries* of October 5, 1888, there is no reason to repeat all this record here, but rather to present only the conclusions drawn from those experiments. A number of articles relating to this were subsequently published in most of the leading English technical journals; but all these are collected in *The Steamship*, published at 2 Custom House Chambers, Leith, Scotland, and need not be reproduced either, the intention in this paper being only to present a general view of the most important facts.

When the idea of the air propeller, or revolving sails for the use of ships, was first originated, I imagined that it, working in the elastic air, ought to be more efficient than the water propeller; experiments proved, however, that the results came as near as possible to the same amount of propulsion—that is, when a water propeller is applied on a vessel, yielding a certain thrust at a certain power, then a two-bladed air propeller, with six times the diameter and with its pitch reduced to something about the half or two-thirds that of the water propeller, substituted therefor on the same vessel, gives the same thrust at a somewhat smaller number of revolutions when the engine power is the same and the weather calm.

When the wind blows at an oblique angle to the air propeller, its effectiveness is increased, because it is then enabled to

work upon undisturbed air. About three-quarters of the winds prove to give increased power. As soon as there is wind this power is utilized if the pitch of the air propeller is changed accordingly.* The wind, when straight against the course, retards the speed, although not very much. Suppose a storm blowing with the speed of 60 ft. per second, and let us also consider a revolving speed of 60 ft. per second given to the points of effort of the revolving sails, in which points the whole pressure is concentrated; then the resultant wind will strike the sails under an angle of 45°. In the course of a year, in our latitudes, there is not a wind strong enough to prevent an air propeller, driven with only 1 H.P., to go

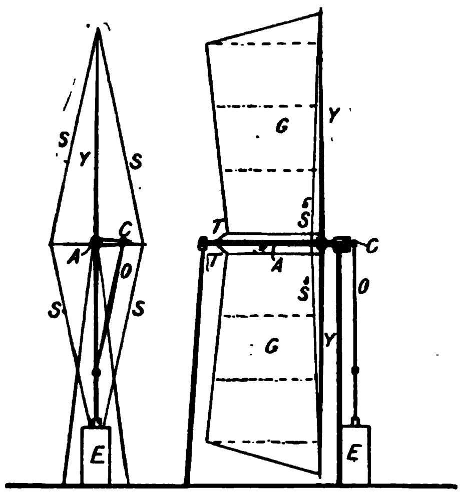

Fig. 1.

straight against it, and three points from the wind its power results in a gain.

Let us, for the sake of estimating the influence of the natural wind, consider the same blowing with a speed, V, and let the points of effort of the revolving sails possess a speed, W; then in a two-bladed propeller, as in the above sketch (fig. 1), one blade will be working against a component, V_c of the natural wind, while the opposite blade will be working with the same. The aggregate influence on both blades will, therefore, be respectively a function of $(W + V_c)^2 + (W - V_c)^2 = 2$

* But of course only when this propeller is mounted on a ship, not when mounted on a balloon driving with the wind.

$(W^2 + V_c^2)$, from which expression the considerable influence of the natural wind is seen, the increase derived from the natural wind being $2\ V_c^2$. Even when the blades are passing the horizontal position, the influence of the natural wind is great, because the normal pressure depends much more on the speed of the air than on the angle of incidence; we only need remember that an angle of incidence of 15° gives a normal pressure, which is half as great as when the angle of incidence is 90°. Quite 80 per cent. of the different wind directions, when sailing in a circular path, are a benefit, whereas nearly 20 per cent. do some harm to the progress.

The best material for making an air propeller is thin steel plate, which enables the highest efficiency to be reached; but it is often a mere chance to hit the best shape. A true mathematical screw surface is, for instance, very inferior, whereas a shape such that *sections through the blades form a feeble curvature, similar to that of an albatross's wing*, is very successful. The only feature in this shape resembling that of a screw is that sections through the blades, parallel with the axis, should have their angles with a plane perpendicular to the axis, decreasing proportionately with their distances from the axis. It being so difficult to obtain correct shapes in steel plates, it is recommended to use canvas covered with oilskin in the following manner: $y\ y$ is a yard fixed in its middle perpendicularly to the shaft, A; the two sails S are stiffened by means of thin battens or booms, put in pockets in the sails, which are fastened to the said yard $y\ y$ by means of buttons working in a groove made in the yard; the pitch of the sails can be regulated by means of elastic sheets, t; s are stays to support the yard y. The whole system is turned by means of a crank, c, and a connecting-rod, o; the vertical engine is indicated by E; *the crank c must be pependicular to the yard y*, because the greatest influence of the natural wind will just take place when the sails are perpendicular. For the sake of not straining the bolt ropes of the sails too much, extra ropes are fastened between the "noks" (end of yard-arm) of the said booms. A close fit between the sails and the yard $y\ y$ is essential; also the canvas should be doubled or tripled, according to the strength required, and covered with oilskin or caoutchouc to make it as impermeable to the air and as smooth as possible, to reduce friction.

Propellers, both in air and water, do their work by creating a rarefaction—*i.e.*, by diminution of pressure or partial vacuum on the drag or rear side of their blades. This was demonstrated by leading a tube from the rear of the side of the blade of an air propeller to the hollow shaft on which it (for this purpose) was mounted, the hollow shaft again communicating with a gauge; nearly the whole thrust was thus found to result from the rarefaction *on the rear side of the blade*. The two agents in operation to create this rarefaction are, first, the suction from the rush of air over the drag or leeward side of a blade; second, the centrifugal force. As the pressure on the thrust side of a revolving propeller blade decreases from the

tips toward the center, the air must, when the shape is correct, move inward toward the lower pressure near the center, with a speed proportioned to the difference in pressure on the blade between its outer and inner parts; the centrifugal force cannot, therefore, rarefy the air on the thrust side of a blade; but exactly the opposite takes place on the drag side of a propeller blade, where the centrifugal force therefore assists in rarefying the air. Something like a little storm center is thus created in front of the propeller, wherewith there is obtained, as it were, a grasp on the ocean of air in front of it, and a huge momentum of air is brought in motion toward the propeller; part of this air passes through and is then acted upon by the thrust sides of the blades. The rarefaction is so intense at high speeds that the air is even literally drawn toward the propeller. An experiment relating to the influence of the rarefaction is published in *The Engineer* of February 6, 1891, and more completely in *The Steamship* of March 2, 1891. It is there explained in what manner the efficiency of a small two-bladed steel propeller, weighing 0.35 lbs., diameter and average pitch 1 ft., area 25 sq. in., was determined, the same being found to ascend 200 ft. into the air when given 70 revolutions per second.

The determination of the efficiency is too intricate to enter on here; but one curious phenomenon—namely, negative slip—is easily demonstrated: The moment of inertia I of the small propeller was 0.0012, and the angular velocity W at 70 revolutions per second was 440 ft. per second, so that the energy $\frac{1}{2} I w^2$ became $\frac{1}{2} \times 0.0012 \times (440^2) = 116$ foot-pounds. The whole of this energy could not, however, be used in flying up, because the propeller hovered at 13.5 revolutions per second when the highest point was reached, corresponding to an amount of energy equal to $\frac{1}{2} I w^2 = \frac{1}{2} \times 0.0012 \times (84.7)^2 =$ 4.3 foot-pounds; the whole amount of energy at disposal for lifting the weight is consequently 116 − 4.3, or about 112 foot-pounds. The propeller, weighing 0.35 lbs., consumed, in flying up to a distance of 200 ft., 200 × 0.35 = 70 foot-pounds, or about 63 per cent. of the energy stored in the propeller. The mechanism through which the revolutions were imparted to the propeller consumed considerable work in friction, etc.; so it was found, through experiment, that a man must develop about 130 foot-pounds in a single pull to give the propeller 70 revolutions per second. The speed was easily measured, and amounted to more than 100 ft. per second, especially while rising between the heights of 30 and 130 ft. from the ground, which distance was passed in much less than one second; whereas, in accordance with the average pitch, equal to 1 ft., the speed should not have exceeded 70 ft. per second. The negative slip was, therefore, considerable in this case, when measured in relation *to the average pitch;* but when air propellers were used for driving boats, and consequently had a comparatively greater resistance to surmount, the positive slip became often three times greater than with propellers in water, and still the efficiency was over 63 per cent., thus show-

ing that the slip had nothing to do with the efficiency of a propeller.

To prove negative slip in the air in another manner, Major Elsdale undertook the following experiment: A propeller was constructed with blades of such shape that their thrust sides became parts of a plane perpendicular on the shaft, while the drag sides formed an angle with the thrust side. Fig. 2 shows a section through a blade, the shaft being represented by A, the thrust side perpendicular to the shaft by T, so that its pitch is equal to zero. A propeller of this type gave a thrust nearly as great as when the thrust side T became parallel to the drag side S, the blade revolving as shown by the arrow.

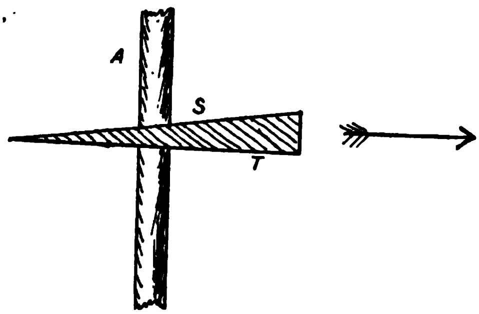

Fig. 2.

When a propeller revolves quickly, the rarefaction often corresponds to a difference in pressure of several inches of water, and the currents produced by centrifugal force seem to prevent the air from striking the drag side, which it would do when negative slip occurs. Mr. Phillips had formerly mentioned the same experiment and explained how he drove a boat with a similar propeller, and as the pitch of the thrust side was equal to zero, the negative slip was infinite in relation to that side.

When an air propeller is required for any purpose, it has already been mentioned that its diameter should be about six times the diameter of a propeller in water, determined for the same thrust; but area, pitch, revolutions, etc., can also be found directly from a model experiment by means of the following formula: Two ships, or, in the case to be considered, two propellers, are said to move with corresponding speeds H and h, when $\frac{H}{h} = \left(\frac{D}{d}\right)^{\frac{1}{2}}$, where D and d are similar lineal dimensions and H and h are the speeds of similar points on the

propellers—for instance, at their circumferences ; under these conditions of speed, the thrust T and t of the propellers, with areas A and a, are in the relation :

$$\frac{T}{t} = \frac{A\,(H)^2}{a\,(h)^2} = \frac{D^3}{d^3};$$

$$\frac{A}{a} = \frac{D^2}{d^2} = \left(\frac{T}{t}\right)^{\frac{2}{3}}$$

results the important equation : (1) $\frac{A}{a} = \left(\frac{T}{t}\right)^{\frac{2}{3}}$, and by means of $\frac{D^3}{d^3} = \frac{T}{t}$ we obtain $\frac{D}{d} = \left(\frac{T}{t}\right)^{\frac{1}{3}}$, so that $\frac{H}{h} = \left(\frac{D}{d}\right)^{\frac{1}{2}}$ gives : (2) $\frac{H}{h} = \left(\frac{T}{t}\right)^{\frac{1}{6}}$, which is the second important equation. The two equations $\frac{A}{a} = \left(\frac{T}{t}\right)^{\frac{2}{3}}$ and $\frac{H}{h} = \left(\frac{T}{t}\right)^{\frac{1}{6}}$ are derived under the assumption that the thrusts vary proportionately with the area and with the square of the speeds, and we are now able to find the revolutions, area, diameter, etc., of any propeller, when we know these qualities from the model. Let it, for instance, be required to construct a propeller able to yield a thrust of 1,000 lbs., which is the resistance of a ship of about 1,000 tons at a speed of four knots ; then, to determine its number of revolutions and the power required to drive it, a model experiment is necessary. To this end a two-bladed air propeller quite 5 ft. in diameter, 4 sq. ft. area, pitch $\frac{3}{8}$ of the diameter, was driven by the power of a man to c. 5 revolutions per second, and gave a 20-ft. boat a speed of 4 ft. per second in calm weather ; the resistance of the boat or thrust of the propeller, at that speed, being 9 lbs., and the brake H.P. on the shaft became $\frac{1}{5}$ H.P., which consequently corresponds to 45 lbs. for each H.P. The area A of the large propeller (which, strictly speaking, should move at a corresponding speed to be of the same efficiency) is then : $A = a\left(\frac{T}{t}\right)^{\frac{2}{3}}$; as the area a of the model is $a = 4$ sq. ft., we get $A = 4\left(\frac{1000}{9}\right)^{\frac{2}{3}} = 4 \times 23 =$ 92 sq. ft. for the area of the large propeller, intended for a

thrust of 1,000 lbs.; and as the area of the model propeller is $\frac{1}{4}$ of the disk area, the same must be the case with the larger similar propeller, whereby its diameter becomes 24 ft. The velocity in the circumference of the small propeller was 75 ft.; the velocity in the large similar propeller will therefore be

$$H = h\left(\frac{T}{t}\right)^{\frac{1}{3}} = 75\left(\frac{1000}{9}\right)^{\frac{1}{3}} = 75 \times 2.19 = 164 \text{ ft. per sec-}$$

ond, which corresponds to 2.2 revolutions per second.

When the corresponding speeds for model and large propeller are termed h and H, and the thrust of the model propeller is 45 lbs. per H.P., then the power to drive the large propeller is $\left(\frac{1000}{45}\right)\frac{H}{h}$, and as $\frac{H}{h} = \left(\frac{1000}{9}\right)^{\frac{1}{3}} = 2.19$, we obtain the H.P. equal to $22 \times 2.19 = 48$; that is to say, the H.P. is 48 if the large ship moves with a corresponding speed to that of the model, which is $4 \times 2.19 = 8.76$, or quite 5 knots; as the large ship is only intended for 4 knots in calm weather, the power will be somewhat reduced; moreover, the efficiency of the large propeller is greater than that of the smaller, which also tends to reduce the power.

Of greater importance is, however, the fact that the resistance of the air varies at a much higher power than that of the square, especially when a surface revolves round an axis in its own plane.* The resistance also increases more than proportionally with the increase of the area when the speed is unaltered. It is not difficult to take these matters into consideration, but it makes the formula more complicated than is suitable for this paper; let it therefore be sufficient to say that the power in this case would be less than 40 H.P., and 2.2 revolutions per second would scarcely be reached, at that power, with a greater diameter than 20 ft.

Several experiments were made with boats furnished with revolving sails or air propellers, as explained in the articles referred to; the largest of these was with a large steam launch belonging to the Royal Dockyard, in Copenhagen, and furnished with an air propeller 20 ft. in diameter. Any of them could, of course, have been used as unit or model for the example given; but when a model experiment is required, it is not always convenient to drive a propeller with steam for that purpose. It is not at all difficult for a man to drive a very light boat at a speed of 4 knots, or about 7 ft. per second; but the model air propeller to be tested must be removed to different boats until one is found which offers the required resistance at a certain speed.

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* When the speed of the points of effort of a surface, revolving round an axis in its own plane, equals that of the same surface when moving after a straight line perpendicularly on its own plane, then the resistance of the revolving surface is about three times greater, on account of the rarefaction produced through centrifugal force.

THE ELASTIC-FLUID TURBINE, A POSSIBLE MOTOR FOR AERONAUTICAL USE.*

BY J. H. DOW, CLEVELAND, O.

THOUGH the successful flying ship is not yet built, and may be long in coming, yet the mechanical possibility that the air may be navigated has been demonstrated, and possibility is sure prophecy of eventual attainment. We already know that the screw propeller aided by the aeroplane could sustain the flying ship in the air, and *scale*† it ahead faster than the highest railroad speed, provided that a reliable motor could be found whose output of power compared with its own weight would but slightly exceed the highest result already reached experimentally.

The compound turbine, utilizing the expansive force of steam or some other elastic fluid, has some characteristics which eminently fit it for aeronautic propulsion, and the possibility that it may sometime be developed to meet *all* requirements is my excuse for this paper.

One of my steam turbines was shown at the Columbian Exposition, in the exhibit of the Hotchkiss Ordnance Company, Limited. The turbine is attached to the launching tube of the Howell torpedo, and is used to store a half million of foot-pounds of propelling power in the torpedo fly-wheel by speeding up the fly-wheel to 10,000 rotations per minute. Steam is not applied to the turbine in the exhibit.

I have only approximate data of the torpedo turbine. It weighs about 105 lbs., and when running at a speed of 10,000 rotations per minute, it develops, non-condensing, a little rising 25 H.P. with steam at 120 lbs. above atmospheric pressure. It has been run at a pressure exceeding 150 lbs., and might safely carry 200 lbs. The power developed at different pressures, speed remaining constant, has been found by careful tests to be about in the ratio of the pressures.

I have a turbine, shown in the accompanying engraving‡ which is a duplicate of the torpedo turbine, except that it stands upon its own base, and has self-lubricating bearings, and varies slightly in the lay-out of the turbine buckets and guide-plates. It weighs, base included, 139 lbs. It was at one time coupled directly to a special dynamo, and although the dynamo proved inadequate to the speed, and doubtless wasted considerable power, yet an electric current was generated of 93 ampères and 83 volts, equal to 10.3 E. H.P. while the turbine and dynamo were running at about 10,000 rotations per minute and using steam at 67 lbs. steam-gauge pressure. The actual power of the turbine itself at that trial, assuming that it equalled the power of the torpedo turbine at like steam pressure and speed, was a fraction over 13 H.P.

* Paper read at Conference on Aerial Navigation, August 1, 1893.
† As boys "scale" a flat stone in the air.
‡ See p. 124.

If the dynamo would have borne the test at 12,000 speed, which is nearer the normal speed of this turbine, somewhat more power would have been developed under the same pressure of steam, and 200 lbs. steam pressure would develop in like ratio fully 40 brake H.P., which would be 1 H.P. for every 3¼ lbs. of this turbine's weight. But this particular turbine was designed and built with an excess of material in order to bear the roughest service. If the design were simplified and all surplus metal were cut away, the weight might be reduced to less than two-thirds of the present figure—perhaps to one-half—and then if an alloy mainly of aluminum were used, instead of bronze as at present, it would not be impracticable even in the present crude stage of steam turbine construction to build a 40-H.P. turbine which would not weigh more than 50 lbs.

However, I must acknowledge that this wonderful output of the turbine is *at present* neutralized, and its value for aeronautical purposes is rendered merely prospective by reason of its comparative wastefulness of steam and the consequent necessity for a large boiler. Yet great improvement in the efficiency of the turbine is even now in sight. Hitherto I have been compelled to lay out the buckets and guide plates of the turbine with primary regard to their easy construction in a common machine shop, but if the opportunity should ever come to me to construct special tools, I would then discard the present zigzag lay-out of buckets and guide-plates and would make them more nearly after the approved curves of the modern water turbine. This change, together with strict regard to the proper ratio of cross-sectional area of vents in the successive compoundings, would greatly improve the efficiency.

And the economy of steam would be still farther improved by the use of a multi-wheel turbine constructed according to my patent, No. 496,352. In this type of turbine the steam works through a succession of wheels strung upon a single shaft, and expansion from the highest initial pressure ever used may be utilized to its extreme limit. Such a motor will weigh more than the two-wheel type and will be more expensive to build, but the saving in weight of boiler and fuel will more than compensate for the added weight and cost of the motor itself.* Theoretically the multi-wheel steam turbine may be as economical of steam as the highest type of steam-engine, and I am of opinion that it will yet closely approach to that degree of efficiency.

The chief difficulty, among the many which have impeded the development of the steam turbine, has been that its speed is necessarily extreme. The turbine buckets must have nearly one-half the velocity of the outflowing steam, or power is wasted. The turbine itself can now be made to run satisfactorily at the required speed, but I have not yet satisfactorily transmitted its power to common machinery. I have run a

* *The Iron Age*, issue of June 1, 1893, has cuts and a description of my multi-wheel turbine. But the lay-out of buckets and guide plates shown there can be improved when special tools of construction are available.

turbine having steam wheels of 5¾ in. diameter (or 1 in. smaller than the wheels in the accompanying engraving), at a speed of 21,000 rotations per minute, and for many days in succession have transmitted its power, amounting to several H.P., through a train of gears to ordinary line shafting. But the fast pinion of the train was short-lived, and the use of gears for this purpose, though possible, is hardly practicable; but the full-speed turbine may be coupled to a like speed dynamo. Without doubt such a dynamo may be developed, which in lightness as related to output will fully match the turbine, while its power may be transmitted to an electric motor running at any other speed of rotation.

If, then, the future flying ship shall be lifted and propelled by two oppositely rotating concentric windmill wheels of large diameter and superlative circumferential velocity, as now seems probable, the respective circumferences of these wheels need only to be mounted in field magnet and armature relation and they will constitute the electric motor. The enormous velocity across the lines of force would require correspondingly diminutive weight of magnet cores and coils, and such a combination of turbine, dynamo, and electric motor for aeronautic propulsion would be unapproachable by any present method in lightness relative to output of power, because it would utilize to the utmost the two factors, *constant strain to the elastic limit* and *unslacking speed to the limit of cohesive strength*, thereby developing *the maximum of power by use of the minimum of material.* As illustration, the main shaft of the turbine shown in the accompanying engraving is only ⅝ in. diameter, yet its capacity for transmitting power at 12,000 speed equals the capacity of a 2½ in. shaft running 187.5 revolutions per minute, although the weight of a ⅝-in. shaft per lineal unit is only one-sixteenth that of the 2½-in. shaft.

But 12,000 is not the limit of turbine speed. As already stated, I have run a 5¾ in. wheel at more than 20,000 speed. Once, on a spurt, it was run smoothly at a measured speed of 35,000 revolutions per minute. Should such prodigious speed be achieved also in the dynamo, or should dynamo and electric motor give place to a more direct method of power transmission from the turbine to the flying mechanism, it is apparent that the output of power relative to weight would be greatly in advance of the estimates which I have given.

Thus far this discussion has dealt with the *steam* turbine exclusively, because my experience has been only along that line; but the value of a motor to be used for aviation depends upon its output of power relative to the *aggregate* weight of motor and all its appurtenances. Viewing the subject in this light, it is doubtful whether *any steam motor* would be satisfactory, because of the heavy supply of water required for the boiler. If I were experimenting primarily for automobile aviation I would not use steam, but would substitute some other elastic fluid. Volatilized naphtha or petroleum mixed with atmospheric air and driving the turbine by a succession of explosions might be highly successful. Indeed, the ab-

sence of stuffing-boxes, or piston-packing, or frictional contact of the heated parts, would give to the gas turbine decided advantages over the piston engine using explosive gases, while the advantage of lightness due to extreme speed would be as great, in comparison with the common gas-engine, as the steam turbine shows compared with the common steam-engine.

Pardon me that I add, civilization will undergo a crucial test of its moral stability in the tremendous economic revolution which *must* attend the successful introduction of aerial navigation, when everybody the world over shall become neighbor to everybody else, and neither fortifications, nor battle ships, nor Chinese walls, nor cordons of customs officers can offer any barrier to enemies or intruders.

But the world is safe for the present; great inventions do not appear until the moment when civilization becomes ripe for them. The flying ship is a certainty of the future, and with equal certainty must come the moral climax of universal brotherhood.

God speed the day.

APPENDIX.

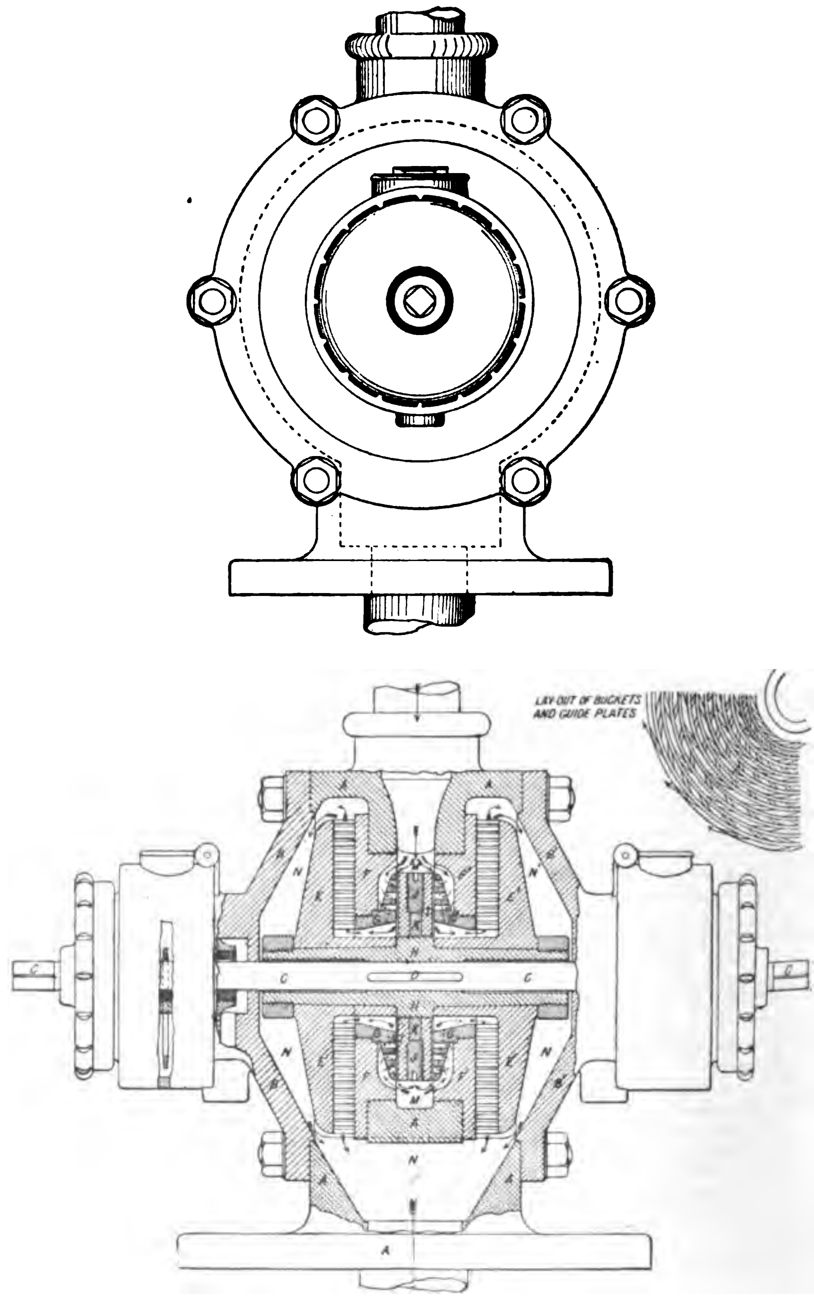
DESCRIPTION OF THE DOW TURBINE.

The body of the turbine case,* *a*, is a short three-chambered cylinder lying on a rectangular base. The middle chamber, *m*, carries live steam, the two outside chambers, *n n'*, take the exhaust steam. The hubs of the case cover, *b b'*, contain the journal-boxes for the driving-shaft, *c*. A sleeve, *h*, is mounted upon the driving-shaft, within the case, and reaches almost from cover to cover. Two steam wheels, *e e'*, are mounted on the sleeve, one in each exhaust chamber, and each steam wheel carries on its inwardly presenting face seven concentric series of turbine buckets which intermesh with corresponding series of turbine guide-plates set upon the outwardly presenting face of each of the fixed disks, *f f'*, which constitute the end walls of the central chamber.† A balancing disk, *i* (explained later), is mounted upon the middle of the sleeve, and runs with narrow clearance between the two balancing face-plates, *g g'*, which are screwed into the fixed disks.

The steam flows as indicated by the arrows. It flows radially inward each side of the balancing disk in the narrow space between disk and each face-plate. (The face-plates also have perforations in radial rows of holes, and radial grooves between the rows, as shown by the dotted lines on the section elevation, to give larger passage for the steam without widening the clearance between the balancing disk and the face-plates.) The further passage of the steam is along the annular space between each steam wheel-hub and the bore of the balancing face-plate surrounding the hub, onward to the guide-plates and buckets, and through these it works its way radi-

* See engraving, next page.

† See lay-out of buckets and guide-plates in the engraving. The present lay-out differs slightly from the engraving.



ELEVATION ON SECTION OF DOW'S ELASTIC-FLUID TURBINE.

ally outward, expending its expansive force, and escapes at the circumference of each steam wheel into the exhaust chambers, and thence through the base of the case into the exhaust-pipe and the atmosphere.

A very slight end play (about three thousandths of an inch in this turbine) is given to the sleeve upon the shaft to which it is feathered, so that the bucket faces may be carried just clear from the fixed disks. Whenever any disturbance pushes either steam wheel too close to its seat, the balancing disk is also pushed from its central position between the face-plates and gives wider steam passage to that steam wheel which is too close to its seat, at the same time choking the passage to the other wheel, thus forcing the wheels back to their normal position.

The journal bearings are self-lubricating, by means of the well-known device of the lubricating ring, *l*, whose internal periphery rolls upon the rotating journal and delivers to it the oil which the ring has brought up from the reservoir below.

The steam wheels and central disk are balanced by an instrument contrived for this special use which is accurate to a single grain. Each piece is balanced independently on the shaft, so that the running balance of the assembled parts is so nice that neither jar nor heating of journals is experienced when the turbine runs at full speed.

NOTES ON THE MATERIALS OF AERONAUTIC ENGINEERING.

By Robert H. Thurston.

Among the essential elements of progress in engineering, and in the constructive arts by which it is sustained, has always been recognized advancement in the discovery and improvement of the materials employed in the building of machinery and of structures demanding a union of strength with lightness, a combination of power with portability, or the concentration of active energy or of maximum resistance to applied forces within minimum volume and weight.

With the progress of the present century, especially in the construction of the heat engines and of steamships and locomotives, and in the endeavor to attain more than the now recognized limit of speed, both on land and at sea, at minimum expense and within minimum space, the demand for materials both light and strong has become constantly more and more urgent, and the engineer and the constructor of to-day are by no means satisfied with those now within reach, notwithstanding the fact that cast irons may be had of a tenacity of 30,000 lbs. per square inch of section ; wrought irons of from 50,000

to 60,000, and steels of any quality that may be desired, from the quality of the best wrought iron up to those having twice that strength, and, in small sections, up to 200,000 or even 300,000 lbs. tenacity; while alloys may be had enormously stronger, tougher, and more trustworthy than had ever previously been known.

The most urgent demand for strong materials of construction comes from the designers and builders of machinery which must be transported, and especially when it must take part in its own transportation—as in the case of the locomotive and the marine engine—while the lack of still stronger materials than those we now are able to produce is one of the greatest obstacles to-day obstructing the engineer and the mechanic in the endeavor to enter upon a new and most attractive field—that of aeronautics.

No matter how ingenious the inventor or skillful the maker, aeronautic progress is impossible (except floating by aid of supporting balloons) until propelling machinery can be produced powerful enough to lift and impel itself and its enclosing air-ship; or, otherwise and perhaps better stated, until such machinery can be built light enough to sustain itself with its load, with a large reserve of lifting and impelling power.

The locomotive hauling our express trains weighs 100 or 150 lbs. per H.P., and it takes several times its weight along the rail at the rate of 50 or 80 miles an hour; the marine engine occupies one-fifth the weight and space of the containing ship, and weighs from 500 and 600 down to as low as 300 and 200 lbs. per H.P.; while torpedo-boat machinery and that of fast yachts weighs, in many cases, as little as 100 lbs., and even, in a few cases, about one-half that figure; but these are all vastly higher than the maximum weights required by the aeronaut in the solution of his great problem. The birds weigh certainly no more than 20 or 25 lbs. per H.P., and man must rival and surpass them. Machinery weighing 50 lbs. to the H.P. is too heavy for his purposes, and 25 lbs. is probably the practical limit for a beginning.

A good draft-horse weighs about 1,500 to 2,000 lbs. per H.P., and ordinary work is done at the rate of less than 1 H. P. per ton of horse. Strong men work at the rate of from one and a half to three millions of foot-pounds per day of eight working hours, or at the rate of about 1 H.P. per 750 lbs. weight as a minimum, and not far from double that figure as a maximum. The animal's muscular substance is subjected to a maximum stress of not more than about 100 lbs. per square inch—in ordinary work, 15 or 20; and this animal, having a density of about one-eighth that of the metallic motor, his rival, as a machine, is obviously entirely unfitted for use as motor where concentration of bulk and weight are necessary. The store of energy on which it draws, its food, is also bulky and weighty, storing as it does but a small fraction of that found in the same weight of coal; and the vehicle propelled, including motor and supplies, must, therefore, be exceedingly cumbersome even in comparison with those in common use for gen-

eral purposes and where weight and bulk are not serious objections.*

Mullenhoff reports the stress per unit section of a bird's muscles as but about 1.2 to 1.4 kilogram per square centimeter, or not far from 15 to 20 lbs. per square inch. Pénaud, on the other hand, gives the resistance of a feather as about 32 kilograms per square millimeter, or 45,500 lbs. per square inch—about the strength of the softer grades of bar and sheet iron. But its density is about 1.3, thus making its value for aeronautic purposes very great, as giving six times the carrying power per unit of weight. The quotient of tenacity by weight is, approximately, 600. Bamboo of good quality exhibits about the same value, but varies, according to Pénaud, from 30,000 to 50,000 lbs. tenacity, and gives a coefficient ranging from 400 to 650;† its variability compensating its occasional superiority. Its density is 1.2.

The animal machine would seem to have a very high factor of safety if we compare these figures with those given by the authorities for the actual stresses on the muscular fiber. Weber makes the frog exhibit but 692 grams per square centimeter; Koster gives 1,087 for the human muscle, and Marey and Plateau give 1,200 and upward for the bird and the insect.‡ But these stresses are probably stated much too low. Marey's highest figures are but 1,300 and 1,400 grams per square centimeter, or but 18 to 20 lbs. on the square inch.§ It is most wonderful that, with these low stresses, with their high factors of safety, and with comparatively bulky and weighty fuel, these animal machines should be able to lift into the air a load equal to 50 per cent. of their own weight, and to traverse a thousand miles without stop in a day. It seems very probable that a weight of somewhere about 10 or 12 lbs. per H.P. for the machinery of propulsion must be worked for if success is to be either certain or satisfactory in this particular detail of the problem which the aeronaut seeks to solve as a would-be "aviator."

This reduction of weight may be accomplished by advances in either or all of three directions: (1) Reduction of weight of material by improved design and proportions; (2) increase of ratio of strength of the materials used to their density; (3) increased velocity of motion of the moving parts of the machinery transmitting energy.

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* It is, however, an extraordinary and interesting, perhaps an important fact in the way of suggestion and of stimulation and challenge of research in this field, that the large animal machine, submitting its tension members to stresses but a small fraction of 1 per cent. of those employed in the metallic structure, to perhaps, as a fair average, one five-hundredth the working stresses on the latter, should, nevertheless, weigh but about ten times as much per H. P.

† Experiments in compression indicate a much lower value, so that the column crushed by its own weight would be but about 10,000 ft. high, with occasional samples indicating 20,000 ft. or more, but more often denoting a column of 5,000 ft. or less.

‡ Marey's "Animal Mechanism," International Series, D. Appleton & Co., N. Y., 1874, p. 64.

§ *Ibid.*, p. 214.

By careful selection and ingenious combination of the lightest and strongest available materials, Stringfellow, Henson and Hargraves have produced machinery of the minimum weight, and the prize engine of the British Aeronautical Society weighed but 16 lbs. per H.P.; that of Maxim, built of tempered tool steel, weighs, with its boiler, less than 10 lbs. per H.P.; and the engines of Langley, without boilers, are similarly reduced in weight to 6 lbs. per H.P. With tin tubing, silk thread, and steel wire, Hargraves has made a compressed-air engine capable of driving its own framework 10 to 12 miles an hour for some hundreds of yards. Petroleum engines and even steam engines have been used in the impulsion of dirigible balloons; and some of the very best work yet accomplished has been through the use of electric storage batteries by Trouvé, Renard and others. In all these instances the secret of such success as has been attained has been through the combination of the elements above enumerated.

Of the two factors which produce the resultant energy in any case of application of power to useful purposes—force or effort, and velocity of motion of the point of application of that effort—the increase of either of which gives increased power, from the standpoint of the engineer, that which is most desirable, usually, is velocity. Increased effort means, in all cases, increased strength, weight and cost of construction; while the increase of the velocity of operation affects weight, strength and cost comparatively little, as it only compels, in some cases, special construction and modification of design to meet the disturbing or straining action of inertia of parts. For the purposes of the aviator it is important that his machinery shall be of such form that, in the product $U = F\,V$, the factor V shall be made just as high as is possible and yet is safe, correspondingly reducing F and proportionally diminishing the weight of the machine producing the energy U. The first necessity, therefore, and obviously, here as in all engineering construction, where machinery is to be produced, is that of securing such a design as will give the largest practicable value to V. When this is done there remains the possibility of reducing the ratio of weight of machine to power developed by reduction of the weight of each element of the machine to the smallest quantity capable of sustaining the effort F transmitted by it; reducing the ratio of density of material to strength, while, it must be remembered, retaining also so much of ductility as is required to insure safety through that resilience which must be present to meet the action of blows and shocks. It is not enough that the material chosen is strong, nor is it sufficient that it shall be light. Aluminium has a density of but 2.6; but its tenacity is but 25,000 lbs. per square inch in sheets, and in fine wire about 60,000; while fine tool steel, ordinarily with a density of 7.8, has a tenacity of 100,000 and even of 150,000 lbs., and in special forms it rises to above 200,000 lbs., and in the form of the watch spring or fine wire to 300,000 and upward. Steel is three times as heavy as aluminium, but it may be given from four to five times its

strength, and is much the better material, so far as we can now see, for aerial constructions. The metal weighing 480 lbs. per cubic foot will sustain, in its strongest form, perhaps 100,000 ft. of its own substance; while the metal weighing but 160 lbs. per foot can uphold only about 25,000 ft. in the form of a suspended bar, or about 60,000 ft. in fine wire. This last is the true gauge of the constructive value of the metal, as of any other substance to be used as a tie. The height of the column which will stand without crushing or bending is the similar gauge of the value of the substance in compression and the span of a standard form of girder which would just sustain itself—that for transverse loading.

The following figures permit this sort of comparison to be made among the various familiar metals of common use in machine construction, as given by Messrs. Hunt, Langley and Hall:*

DENSITY AND WEIGHT OF METALS.

METAL.	Weight of 1 cu. ft.	Tensile strength per sq. in.	Length of a bar that just supported its own weight.
Cast iron........................	444 lbs.	16,500 lbs.	5,351 ft.
Ordinary bronze................	525 "	86,000 "	9,874 "
Wrought iron	480 "	50,000 "	15,000 "
Hard struck steel	490 "	78,000 "	22,922 "
Aluminium......................	168 "	26,000 "	22,285 "

It is seen that the value of aluminium for purposes demanding maximum strength per unit of weight is substantially the same as that of steel having a tenacity of between 75,000 and 80,000 lbs. per square inch, while both metals are superior to the others in the list and 60 per cent. better than wrought iron, the next best in the catalogue. The tool steels and the special grades of steel having greater strength than that here specified are correspondingly more valuable for such purposes, and, could the highest figures above given for fine steels be attained in parts of needed magnitude, as seems not at all unlikely with further progress in metallurgical invention and discovery, the indications are that, for the working parts of machinery of transportation, the steels will retain their present superiority over all other metals. A steel of 300,000 lbs. tenacity has a value for parts subjected to that form of stress about four times that of aluminum.

For general purposes and, in the opinion of the writer, for aeronautic construction as well, it will probably be found advisable to keep well within the elastic limit of the substance; in which case the tests for compression and for tension members will be substantially the same. Although different

* Trans. Am. Inst. Mining Engineers, 1891.

materials may be often found best suited for the two applications, as silk cord for tension and steel tubes for compression, the requirements of both will be such as make the tension tests and the elastic limit in both cases the real measure of the quality of the material. In what follows, therefore, no distinction is in this respect noted among the various materials discussed.

THE LIMITING VALUE of the materials of aeronautic construction may be assigned by reference to the fact that thus far only the fine steels have been considered sufficiently strong and light to permit the construction of machinery for such purposes. Their cost, when thus employed for all the working parts, and in forms giving maximum strength with minimum weight, is very great; but this is a comparatively unimportant matter for present purposes.* Such steels are capable of sustaining their own substance in lengths of from 25,000 to above 100,000 ft.; and we may take the former figure as our limiting value. Any material incapable of sustaining 25,000 ft.—about 5 miles (7,606 meters)—of its own substance in a pendent bar may be ruled out of the list of those which promise to be of special service in future aeronautic construction.

Similarly, any substance thus incapable of sustaining about 15,000 ft.—3 miles (or 4,600 meters)—without exceeding the elastic limit of the material at the point of suspension, may be also discarded from the list. Still another gauge of the value of the material is the quotient of its tenacity by its density. If the tenacity in pounds on the square inch, divided by the weight in pounds per cubic foot, is less than about $\frac{t}{w} = 150$, it is not wanted; or if the quotient, elastic resistance divided by weight, is less than $\frac{e}{w} = 100$, it will not suit such purposes.

The limiting value is, in fact, in most cases, if not in all, the elastic limit of the material. The elastic limit of the substance never has the same relation to the ultimate strength in any two materials. In the softer metals, as in copper, tin, lead, zinc, there is no definite elastic limit until one has been produced by load, the metal stretching sensibly with every sensible load, however small. Iron and steel have a definite elastic limit at their first loading; but it rises with every load in excess of the "primitive" elastic limit, until finally the elastic limit coincides with the ultimate resistance, and the piece then breaks without the occurrence of extension marking the separation of the two critical points now united. The process of distortion of these metals up to the point of fracture is thus a process of production of a series of what the writer has

* Mr. Maxim informs the writer that his 300 H. P. engine, built largely of tool steel, with many parts subject to compressive stress turned and bored to give columns of maximum efficiency, has cost about its weight in silver.

called "normal" elastic limits, and the "autographic diagram" of the process devised by him, as exhibited by his "autographic recording testing machine" of 1872, is simply a curve, self-produced, graphically comparing this series of normal elastic limits with the corresponding stresses of which they are the products.*

FIBER and other organic substances promise great usefulness in the construction of some form of aeronautic apparatus, and have, in fact, in ballooning, been the only forms of material meeting the requirements of that branch of engineering work. Of the available forms of fiber, silk, hemp and flax, with perhaps, in some cases, cotton, are the kinds employed. Of these the strengths of ropes and cords and of fabrics made of the respective substances are nearly in the proportion, according to Labillardière, of 100, 50 and 35 for the three first named. Hempen cable is tested by naval authorities up to about 17,000 lbs. per square inch, according to Haswell, or to one-third the strength of good iron cable of moderate size. As it weighs about one-fifth or one-sixth as much as iron and steel, it has twice the value of the latter, where it can be employed in aeronautics if we take the relative value of materials for this purpose as gauged by their strength per unit of weight. Hemp will support about 75,000 ft. of its own length, according to the latter authority, or three times as great a length as aluminium or as steel of 75,000 lbs. tenacity.

Hemp, iron and steel rope, as made for the market, according to Clark, weigh respectively 3, 2, and $1\frac{1}{2}$ lbs. per fathom, or 0.5, 0.333 and 0.25 lbs per foot for a working load of about 1,000 lbs., or for a breaking load of about 6,000 lbs. Their breaking lengths are on this rating—12,000, 18,000 and 24,000 ft.—and they will sustain without exceeding their working loads, respectively, 2,000, 3,000 and 4,000 ft. On this rating hemp would not answer our purpose.

Trautwine's and other tables give the following :

Material.	Strength.
Glue	500 to 750 lbs. per sq. in.
Good leather	3,000 " 5,000 " "
Whalebone	7,600 " "
Horn	9,000 " "
Manilla	12,000 " "
Hemp	15,000 " "
Ivory	16,000 " "

Only the last two can have much value in light constructions.

BRAIDED LINEN—good fishing line weighing 0.00043 lbs. per foot—tested in the Sibley College laboratories had a breaking weight of 17.05 lbs., and was found capable of sustaining a length of 39,520 ft. It is thus superior by about 50 per cent., thus gauged, to aluminium and to steel of about 0.5 per cent.

* See the writer's "Materials of Engineering." vol. ii. (pp. 340-48, 345, 343, 350, 360, 379-84, 444, 461, 491, 531-46, 591, 595-98, especially fig. 137, 605-15), for extended discussion of this important and interesting subject as affecting iron and steel. In vol. iii. also will be found the same general treatment of the case of the metallic alloys and their constituents.

carbon, and comes into the list of valuable materials for aeronautical work.

SILK is the strongest of all the organic materials available for construction. It has, in its best form, a specific gravity of about 1.3, and is three times as strong as linen and twice as strong, in the thread, as hemp. Its finest fibers have a section of from 0.0010 to 0.0015 diameter. It should sustain, in the form of thread, cord, or woven fabric having well-spun warp and woof, about 35,000 lbs. per square inch of its cross section, and its suspended fiber should carry about 150,000 ft. of its own material. This is six times as large a figure as is found for aluminium and for steel of 75,000 lbs. tenacity, and 50 per cent. greater than is reported as obtained with steel in the form of watch spring or fine wire.

Curiously enough, human hair, and especially that of women, comes next to silk in tenacity; a series of 23 tests made at Sibley College having shown that various samples of human hair broke at such strains as to indicate that they would just sustain from 50,000 to 79,000 lineal feet of their own substance, while silk gut was shown, by similar tests, to carry 90,000 lineal feet, and boat paper 16,800 ft. of their own substance.

"SILKWORM GUT," "sinew," or "gut" simply, as it is variously called, is the product of the secretive glands of the silkworm; the same substance as silk, but in form somewhat different. It is obtained by destroying the worm when just ready to spin its thread, and when the *sericteria* are full of the fluid which later becomes silk. The worm is soaked in vinegar some 12 or 14 hours, and then, when seen to be sufficiently softened, is pulled apart by the operator, breaking the glands open and exposing the mass of glutinous matter which has been secreted within them. He attaches this to a board conveniently placed for the purpose and draws out the secretion until it lies, a somewhat heavy thread, across the length of the board, when he secures it with a pin and runs another fiber across, similarly fixing it. He continues drawing out these threads until he has exhausted the gland. They dry in the sun, become exceedingly hard and very strong, and are used mainly for the "leaders" of trout and other fishing lines. The source of this product is mainly Southern Europe.

"Catgut" is the dried and twisted material of the intestines of various animals. It has considerable tenacity, but much less than the preceding. Its suspended line will break when carrying about 25,000 ft. of its own substance, or about the same length as aluminium and machinery steel.

Rawhide is the dried and untanned hide of domestic animals. It has less strength than gut or sinew. It will carry about 15,000 ft. of its own suspended substance.

Fine "sinew," or silkworm gut, taken from the writer's fly-book, tested in the Sibley College laboratories, weighed 0.0001322 lbs. per foot, and sustained a weight equal to that of 42,240 ft. of its own material, its average breaking load being 5.65 lbs. It is thus superior, where capable of application, to braided linen.

Common "catgut," violin string (E), in the same series of investigations, weighed 0.002097 lbs. per foot, and broke with a load of 75.87 lbs., equivalent to a length of 36,175 ft. This was of fair but not superior quality. It is seen to have somewhat less than the value of linen line.

These fibers thus have about the value of steel at a tenacity of 115,000 to 125,000 lbs. per square inch, and less than one-half that strength which is attainable in the latter in sections similar to those of these natural fibers. We find nothing in nature that can compete, for present purposes, with the finest steels in the form of the finest wire and thinnest ribbon or sheet. No natural substance can be found as yet which can approach the rival metals in hardness and safety against injury by shock or abrasion, both of which qualities are of great importance.

The fabrics woven of the fibers which have been above discussed have substantially the same rating as the fiber of which they are composed. But these fabrics may sometimes have peculiar value for special purposes, and may prove more useful than the otherwise superior metals. Silk fabrics have hitherto been the only materials available for the covering of balloons, for the construction of aeroplanes, and for any purposes which demand the employment of large surfaces of minimum weight combined with maximum sustaining power. They are the best available materials for the purposes of the balloonist or the aviator, employing balloons for complete or partial support. Should the aviator find successful methods of flying without the use of the supporting balloon, they will very possibly still remain the best materials for his wings and planes and screws, as they have for ages been the only reliance of the seaman, whether in fishing boat or most gigantic full-rigged ship.

THE WOODS, combining as they do lightness, stiffness and considerable strength, are often found the very best of all the materials of construction where this combination of qualities is important. Some of the most successful of all attempts to construct aeronautic apparatus have been the result of skillful application of the strong, stiff and light woods in their production. Bamboo, ash, spruce and the pines freest from pitch have been found especially suitable. The tables on following pages give the essential data for all the woods common in our markets.

Across the grain the tenacity is much less, being for the pines and spruce woods from one-tenth to one-twentieth ; and in harder woods from one-sixth to one-fourth the figures just given. In oak it is one-fourth, in pine hardly one tenth.

The crushing resistance of timber is as variable as its tenacity Mean values for good quality only can be given.

The following *moduli of crushing strength* are deduced from experiments upon pieces 1 in. (2.54 centimeters) in diameter, and 2 in. (5.08 centimeters) long.

Hodgkinson found the compressive strength of wet wood to be frequently less than half that of dry.

CO-EFFICIENTS OF TENSILE RESISTANCE.

("Materials of Engineering," Thurston, vol. I.)

	British.	Metric.
	Lbs. per sq. in.	Kg. per sq. cm.
Ash	10,000 to 15.000	703 to 1,055
Birch, Black	7,000 " 10,000	492 " 703
Beech	8,000 " 12,000	562 " 844
Box	10,000 " 15,000	703 " 1,055
California Spruce	12,000 " 14.000	844 " 984
Cedar, Bermuda	4,000 " 7,500	281 " 527
" Guadaloupe	5.000 " 9,500	352 " 668
Chestnut	7,000 " 10,500	492 " 738
" Horse	8,000 " 12.000	562 " 844
Cypress	4,000 " 6,000	281 " 422
Elm	8.000 " 13.000	562 " 914
Fir (New England Spruce)	5,000 " 10.000	352 " 703
" Riga	5 000 " 12,500	352 " 879
Greenheart	6 000 " 9,000	422 " 633
Holly	10 000 " 15,000	703 " 1,055
Hickory, American	10,000 " 14,000	703 " 984
Lancewood	8,000 " 15,000	562 " 1,055
Larch	6.000 " 10,000	422 " 703
Lignum Vitæ	10,000 " 12,000	703 " 844
Locust	10,000 " 15,000	703 " 1,655
Mahogany, Honduras	5,000 " 8,000	350 " 560
" best Spanish	8,000 " 15,000	562 " 1,055
Maple	8,000 " 10,000	562 " 703
Oak, American Live	10,000 "	703 " ....
" " White	10 000 "	703 " ....
" English	9,000 "	633 " ....
" best English	12,000 "	844 " ...
Oregon Pine	9,000 " 14.000	633 " 984
Pear	7,000 " 10.000	492 " 703
Pine, Pitch	8,000 " 10,000	562 " 703
" Red	5,000 " 8.000	352 " 562
" White	8.000 " 7.500	862 " ....
" Yellow	5,000 " 12,000	352 " 844
Plum	7.000 " 10,000	492 " 703
Poplar	7.000 "	492 " ....
Spruce	5,000 " 10.000	352 " .703
Teak	10,000 " 15,000	703 " 1,055
Walnut, Black	8,000 "	562 " ....
Willow	10.000 "	703 " ....

In many cases it will be noticed that the tensile strength of wood is double its resistance to crushing, even in short pieces.

The densities and weights per cubic foot of the woods vary in specific gravity from 0.5 to 1.2 accordingly as they are light and are well seasoned, on the one hand, and heavy and green on the other, and from 30 to 80 lbs. per foot. The latter is the weight of green live oak, which weighs 65 lbs. dry; the former that of yellow pine, which weighs 40 lbs. per foot when well seasoned. White oak weighs from 50 to 68 lbs. according to the state of seasoning. For aeronautical purposes all timber should have been at least a year seasoning, and

should be so treated when in the structure that it cannot absorb moisture.

COEFFICIENTS OF RESISTANCE TO CRUSHING.

(In direction, parallel with fibers.)

("Materials of Engineering," Thurston, vol. i.)

	British.	Metric.
	Lbs. per sq. in.	Kg. per sq. cm.
Alder	6,000 to 7,000	422 to 492
Ash	4,600 " 8,000	323 " 562
Beech	8,000 " 9,000	562 " 633
Birch	6,000 " 10,000	422 " 703
" English	5,000 " 6,500	352 " 457
Box	8,000 " 10,000	562 " 703
Cedar	4,000 " 6,500	281 " 457
Cherry	5,000 " 6,500	352 " 457
Chestnut	4,000 " 4,800	281 " 337
Elm	8,000 " 10,000	562 " 703
Greenheart	10,000 " 14 000	703 " 984
Hickory	8,000 " 9,800	562 " 689
Larch	8,000 " 5 500	211 " 387
Locust	7,500 " 9,500	527 " 668
Lignum Vitæ	8,000 " 9,600	562 " 675
Maple	5,000 " 6,000	352 " 422
Mahogany, Spanish	7,000 " 8,000	492 " 562
Oak, English	6,500 " 10,000	457 " 703
" Live	8 000 " 10,000	562 " 703
" White	5,500 " 8 000	387 " 562
Pear	.... " 7,500	387 " ..
Pine, Red	6,000 " 7,500	422 " 527
" White	8,000 " 6,000	211 " 422
" Yellow	6,500 " 10 000	457 " 703
Spruce	4,500 " 6,000	316 " 422
Teak	6,000 " 10,000	422 " 703
Walnut, Black	5,600 " 7,000	394 " 492
" White	7,500 " 9,000	527 " 633
Willow	8,000 " 6,000	211 " 422

The relative resilience of timber is given by Haswell as below:

MATERIAL.	Value.	MATERIAL.	Value.
Ash	1.0	Larch	0.84
Beech	0.86	Oak	0.63
Cedar	0.66	Pitch Pine	0.57
Chestnut	0.73	Spruce	0.64
Elm	0.54	Teak	0.59
Fir	0.40	Yellow Pine	0.64

The same authority gives the following as the relative values of the woods under compression in long columns :

Material.	Value.	Material.	Value.
Ash	3,571	Oak, Quebec.	2,927
Beech	3,079	Spruce	2,522
Cedar	700	Sycamore	1,833
Elm	3,468	Teak	6,555
Oak, English	4,074	Walnut	2,878
Mahogany	2,571	Yellow Pine	2,193

Cast iron, oak and pine have the relative standing, irrespective of weight : 10,000, 1,088, 785.

The weights per cubic foot and the densities of seasoned woods of most promise for light and strong constructions are the following :

Material.	Specific Gravity.	Weight per cu. ft.	Material.	Specific Gravity.	Weight per cu. ft.
Ash	.69	43	Mahogany, Spanish	.72	45
Beech	.66	43	Oak, White	.76	43
Birch	.57	35	" Live	1.07	67
Cedar	.56	35	Pine, White	.42	29
Elm	.57	36	" Yellow	.54	34
Fir (Norway spruce)	.51	32	Spruce	.50	31
Hemlock	.37	23	Sycamore	.62	39
Hickory	.69	43	Walnut, Hickory	.67	42
Lancewood	.72	45	Willow	.49	37
Mahogany, Honduras	.56	35	Yew	.80	50

The strongest of the woods thus has a weight, when well seasoned, of about 40 lbs. per cubic foot, and a tenacity of about 10,000 lbs. per square inch ; the ratio of the latter to the former figure being above 250, they are thus seen to be equivalent, for such constructions as are here considered, to steel of about 125,000 lbs. tenacity. The finest qualities of ash give $\frac{t}{w} = 350$, and are equivalent to steel of about 175,000 lbs. ; the best white pine is equivalent to steel of about the same tenacity ; the best yellow pine, to a trifle higher quality of steel. Hemlock gives $\frac{t}{w} = 400$, and is equal to steel at about 200,000 lbs. tenacity. Hickory closely approaches these latter figures, and it may be asserted that the woods generally of a

better class may be taken as equivalent to the steels of the strongest classes for such purposes as the former best suit.

A survey of the more familiar metals shows that lead, tin, zinc and copper are too weak, in proportion to their weight, to serve as materials of aeronautical construction, except under such circumstances as to compel their use in spite of their unsatisfactory tenacity. This leaves us only, therefore, among those metals, the best grades of iron and steel.

The following table* exhibits with substantial accuracy the mechanical properties of the familiar forms of iron and steel as customarily employed in engineering.

All of these metals except the last, which is not available, in any event, on account of its weakness, may be taken as weighing 0.28 lbs. per cubic inch, or 485 lbs. per cubic foot, the steels having slightly greater densities than the irons, which weigh about 485. As steels are now always available, the irons may be left out of account.

STEEL CASTINGS seem likely to prove peculiarly valuable for use in the framework and massive parts of machinery in which lightness is important. Those made for use in ordnance construction, as for gun carriages, are expected by the Ordnance Bureau of the United States War Department to have a tenacity of not less than 65,000 lbs. per square inch, with a ductility not less than 20 per cent., and less than 55,000 lbs. tenacity; and 15 per cent. elongation in test pieces four diameters long condemns the metal. Castings weighing three or four tons have been made to these specifications and given tenacities exceeding 70,000 lbs. and elastic limit of above 30,000 lbs, with elongations exceeding 33 per cent., and a contraction in area of above 45 per cent. Such metal, especially in castings of parts often difficult to otherwise form, gives promise of becoming enormously valuable in locomotive, marine engine, and aeronautic work. Comparing them with sound aluminium, we find that they may be made to sustain a length of about 25,000 ft., have a ratio of tenacity to heaviness of about 140, and if, as is perfectly practicable, giving somewhat greater hardness and strength for aeronautic than for other construction; and especially as they will usually be employed in small parts or in parts having small sections, they may, by Whitworth's or other method of solidification, be made of considerably higher value than even the above figures would indicate.

Whitworth, with a compression of the solidifying casting under about 20 tons per square inch pressure, obtained castings ranging from 80,000 to 150,000 lbs. tenacity, with elongations ranging from 35 to 14 per cent.†

Similar figures may be obtained by drop forging, and it may be confidently anticipated that the rapid improvement now in progress in the manufacture of this class of metals will soon give us maximum tenacities in masses of large as well as small section, and bring that maximum up to and above the highest yet attained in the smallest sections. It is to-day possible to

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* See next page. † "Materials of Engineering," vol iii., § 289.

QUALITIES OF IRONS AND STEELS.

SOME STRENGTH OF MATERIALS CONSTANTS.	Per cent. Carbon.	Stress at Elas. Lim. Tension.	Stress Ultimate Tension.	Stress at Elas. Lim. Comp.	Stress Ultimate Shearing.	Modulus of Elasticity. Shearing.	Modulus of Elasticity. Tension.
BESSEMER AND SIEMENS-MARTIN STEEL.	.10	35,000	58,000	35,000			
	.15	40,000	65,000	40,000	50,000		
	.20	45,000	70,000	45,000	55,000		
	.50	50,000	80,000	50,000	60,000	10,000,000	30,000,000
	.70	55,000	90,000	55,000	65,000		
	.80	60,000	100,000	65,000	70,000		
	.96	70,000	120,000	70,000	85 000		
Best wrought iron		28,000	58,000	28,000	45,000	10,000,000	28,000.000
Common wrought iron....		25,000	45,000	25,000	32,000	10,000,000	28,000,000
Crucible or tool steel......................		50,000 to 65,000	100,000 to 125,000	60,000 to 80,000		12,500,000	31,000,000
Malleable cast iron.........................		20,000	35,000	22,000			
Good steel castings........................		30,000 to 40,000	60,000 to 80,000	35,000 to 40,000			25,000.000 to 31,000,000 Average, 28,000,000
Good cast iron............................			20,000 to 35,000. 25 000 av.	Ult. Comp. 75,000 to 150,000. 100 000 av.	20,000 to 25,000	8,000,000	15,000,000

secure such castings as the above with a ratio of tenacity to weight of from 150 to 300, and capable of carrying a length of their own section not less than 25,000 and possibly as great as 50,000 ft., and from equality with aluminium up to twice its value. Such castings are, therefore, to-day available for the frames and cylinders and other stationary and intricate parts of the machinery of aeronautics, such as cannot, as a rule, be practically and economically made of the forged metals.

There still remains some, perhaps much work to be done in finding ways of insuring not only the finest qualities and best results in the making of any grade of steel, carbon or other, and especially in securing absolute uniformity of product in every case.

It has already been said that copper, tin and zinc are all too soft to have any value in aeronautic construction ; but some of their alloys might be thought worthy of consideration by the designer. A very extensive and complete preliminary exploration of this field of metallurgical work was made by the writer * some years ago, with the result that the " maximum metal" might be brought up to about 70,000 lbs. per square inch (rather more than the so-called " Tobin bronze"), with a specific gravity of about 8.3, or some 520 lbs. weight per cubic foot. As, therefore, it weighs more and is weaker than the fine steels, and can only sustain about 25,000 ft. of its own substance, such an alloy is inferior to steel, but may possibly find place for some purpose in machinery construction of the class referred to.

We may, therefore, pass to the consideration of aluminium, from which great things have been expected in aerial navigation.

ALUMINIUM, at a density of 2.6, has less weight than other metals in the proportions of 1 to 2.95 for steel and wrought iron, 3.6 for copper, 3.45 for brass, bronze and nickel, 4 for silver, 5 for lead (nearly), and 7.7 for gold, while it is 50 per cent. heavier than magnesium. A sheet of aluminium 1 foot square and 1 in. thick weighs 14 lbs. ; a bar a foot long and 1 in. square in section weighs 1.17 lbs. ; and the metal weighs 0.092 lbs. per cubic inch, or 158.976 lbs. per foot in sound castings. The cubic foot of iron weighs 485 lbs. ; of steel, 490 ; copper, 555 ; brass, 425, and magnesium, 198 lbs. The cost of aluminium has been reduced, since 1886, from $12 to 50 cents a pound, and it has come to be a metal of construction. It melts at about 1.200° F. (700° C.), passing through a pasty stage, like wrought iron, above the red heat. It can be easily welded by the electric current, but is not as yet capable of being satisfactorily soldered or welded by other processes, although it may be thus united to itself by exceedingly careful manipulation. The metal is not volatile in any temperature to which it has yet been subjected. Being unoxidizable under ordinary conditions, it requires no flux in working it. It takes up some silicon in melting in crucibles, and if melted in

* New method of planning researches, Proceedings A. A. A. S., 1877, vol. xxvi., 1878 ; " Materials of Engineering Construction," R. H. Thurston, vol. iii., ch. xi., p. 229, *Journal Franklin Institute*, Feb., 1883.

clay absorbs sensible quantities of that element. A lining of magnesia prevents this action. Heated in presence of chlorine compounds, it unites with that element with avidity.

Aluminium can be rolled either cold or at temperatures between 200° and 400° F., the lower heats being best. It becomes red-short above the upper limit. Cold rolling, hammering and wire-drawing harden and strengthen the metal greatly, sometimes doubling its strength and making it equal to steel of 150,000 lbs. tenacity, and much more valuable for purposes of machine construction when ductility is essential. It is possible that this process, or this method combined with some nicely adjusted dosing with toughening metals, may prove a means of making it useful in aeronautic work. It requires frequent annealing when cold rolled or otherwise worked at low temperature. It anneals like iron and steel, by slow cooling; not, like copper and the brasses, by sudden chilling from a low red heat. Heating in boiling oil and allowing the piece to cool with the liquid answers the purpose well in the case of small articles. Well annealed, it is exceedingly soft, pliable and malleable. Its high coefficient of expansion—0.0000115 on the Fahrenheit scale; 0.0000206 per centigrade degree—which is nearly double that of iron and between those of copper and tin, may prove a disadvantage when exposed to irregular heating or to the heat and shadow of sunlight. It expands and contracts $\frac{1}{75}$ in. per degree of Fahrenheit in 100 ft. For each 100 degrees, which may perhaps be taken as the probable atmospheric range, it would alter its length to the amount of $1\frac{1}{3}$ in. in 100 ft. Its castings have a shrinkage of about a quarter of an inch to the foot. Its malleability comes into play, in the process of increasing tenacity by cold rolling, to such an extent that it is found practicable to reduce its section, if frequent annealing is resorted to, quite as greatly as in similarly working steel.

The following are the figures given as those for aluminium in its various commercial and available forms:

	Lbs.
Ultimate strength per square inch in tension (castings)	15,000
Ultimate strength per square inch in tension (sheet)	24,000
Ultimate strength per square inch in tension (wire)	30,000–65,000
Ultimate strength per square inch in tension (bars)	28,000
Percentage of reduction of area in tension (castings)	15 per cent.
Percentage of reduction of area in tension (sheet)	35 per cent.
Percentage of reduction of area in tension (wire)	60 per cent.
Percentage of reduction of area in tension (bars)	40 per cent.
Elastic limit per square inch under compression in cylinders, with length twice the diameter	3,500
Ultimate strength per square inch under compression in cylinders, with length twice the diameter	12,000
The modulus of elasticity of cast aluminum is about	11,000,000
Elastic limit per square inch in tension (castings)	6,500
Elastic limit per square inch in tension (sheet)	12,000
Elastic limit per square inch in tension (wire)	16,000–30,000
Elastic limit per square inch in tension (bars)	14,000

Alloyed with copper, titanium or silver, the strength of the metal is increased by even very small doses to, in some cases, double that of the unalloyed element. These alloys also have high conductivity—a very important matter if, as seems not at all improbable, electro-dynamic machinery may at some time

be used for aeronautic work.* The conductivity of a wire of titanium alloy of no less than 80,000 lbs. tenacity is reported by Messrs. Hunt, Langley and Hall, as, weight for weight with copper, 170 to 100, and for iron, 170 to 17, or 100 to 10.

THE ALLOYS OF ALUMINIUM have been as yet comparatively little studied, with the exception of the aluminium bronzes, in which the lighter metal is substituted for zinc or for tin; but work is constantly in progress now that the cost of aluminium is reduced to such a figure that the investigation is no longer exceptionally expensive. Aluminium wire can, it is stated, be already supplied at a tenacity of 60,000 to 70,000 lbs. per square inch and of a ductility measured by a reduction of area of 50 per cent., and alloys are predicted having double the former tenacity, and with a density not exceeding 3.5.

Sound castings of the metal have been seen to be capable of carrying about 5 miles of their own substance, and to be equivalent to steel of about 75,000 lbs. tenacity. These most promising alloys have not above 40 per cent. higher density, and possess double strength; they may therefore be expected to sustain about 7 miles, or about 35,000 ft., and thus stand beside the steels of about 100,000 or 110,000 lbs. tenacity. It does not seem probable that these alloys or the metal unalloyed will prove of special value in the construction of the machinery of propulsion; but, in the form of sheet metal, it may prove that they may be utilized advantageously in the construction of the vessels which are to be propelled, whether they be aeronautic, nautic, or terrestrial. The competition seems likely to lie between these alloys and those of other light metals and the various compositions of sheet steel, of paper and of woven fabrics.

Aluminium is often introduced into iron with advantage, apparently acting mainly as flux, as it disappears to a very large extent in the working of the metal, and its purifying action is the main indication, in the finished material, of its earlier presence. It is added in the bath in the puddling furnace in the manufacture of wrought iron immediately before the iron "comes to nature;" it is introduced into the fluid mass in steel making immediately before tapping off, and it is thrown into the cupola at the last moment before pouring, in purifying cast iron. From one-half to two-thirds of the aluminium is lost in the operation, and from one-half to one-third is found in the iron or steel. The admixture is usually about 0.1 of 1 per cent. The gain by its use is stated to be sometimes as much as 20 per cent.

ALUMINIUM BRONZES are usually alloys in which the tin of common bronzes is displaced by aluminium. The percentages of the lighter metal are commonly between 5 and 10; but both larger and smaller proportions have their special value for specific purposes. Their densities range from 8.5 for the "2¼ per cent. bronze," to 8.25 for the "5 per cent. bronze," and to 7.6 for the "10 per cent. bronze" as cast. The worked metals

* It has, in fact, already been comparatively successful.

rise in density to 8.6, 8.3 and 8.85. The best-known and most-used alloy is the 10 per cent. alloy, which has, when sound and pure, a tenacity of about 120,000 lbs. per square inch, an elastic limit at about two-thirds this figure, and an elongation in standard tests of from 25 to 30 per cent., with reduction of minimum section from 25 to 40 per cent. In castings its strength is usually a third less than the above, and its ductility less in an equal proportion. The modulus of elasticity is in the neighborhood of 18,000,000 lbs. In compression, its resistance is from 150,000 to 160,000 lbs. per square inch. With 5 per cent aluminium the alloy has about three-fourths the strength above given and 50 per cent. more ductility. These alloys can be forged like wrought iron or steel, at a full red heat, and are unaffected by high temperatures. Aluminium and its alloys are annealed by slow cooling.

The introduction of copper into aluminium increases its density slowly and its strength rapidly. Each 1 per cent. raises the density about 0.025, the computed increase being 0.06 and the tenacity one-third; 6 per cent. copper giving an alloy of double the tenacity of the pure aluminium, and but 5 per cent. increased density.

The following are the figures obtained from experiments at Chalais :*

ALUMINIUM-COPPER ALLOYS.

Aluminium Per Cent.	Copper Per Cent.	Specific Gravity.		Tenacity, Lbs. per sq. in.
		Estimated.	Actual.	
100	0	....	2.67	26,585
98	2	2.78	2.71	43,563
96	4	2.90	2.77	44,130
94	6	3.02	2.82	54,778
92	8	3.14	2.85	50,374

The maximum effect of the dosing with copper is seen to be found at about 6 per cent. copper, 94 aluminium. It is, however, possible that other changes may occur at other proportions as yet unstudied. The quotient of the tenacity by the weight per cubic foot being, for the best alloys here given, about 300, it is obvious that they may serve our purposes better than the pure metal and even better than steels of above 120,000 lbs. tenacity. The 6 per cent. alloy will sustain a pendent length of its own substance of about 40,000 ft., or nearly eight miles.

The following are data obtained from copper-aluminium

* Appleton's "Cyclopædia of Applied Mechanics," Supp. (Modern Mechanism), 1892, art. Alloys, p. 24.

alloys supplied by Mr. William Cowles to the Bureau of Steam Engineering of the United States Navy Department, and tested at the Watertown Arsenal :*

COPPER-ALUMINIUM ALLOYS.

MARK.	Approximate Composition.	Diameter.	Tenacity.	Elas. Lim.	Elong.	Red. A.
					Per ct	Per ct
1 C..	Cu., 91.5 ; Al., 7.75 ; Si., 0.75...	1.875	60,700	18,000	23.20	30.2
7 C..	Cu., 88.66 ; Al., 10 ; Si., 1.33....	1.875	66,000	27,000	8.80	7.80
9 C..	Cu., 91.5 : Al., 7.75 : Si., 0.75...	1.875	67.600	24,000	13.00	21.62
10 C..	Cu., 90 ; Al. 9 ; Si., 1	1.875	72,830	33,000	2.40	5.78
11 C..	Cu., 63 ; Zn., 33.33 ; Al., 3.33 ; Si., 0.33......................	1.875	82,200	60,000 to 73,000	2.33	9.88
13 C..	Cu., 92 ; Al., 7.5 ; Si., 0.5......	1.875	59,100	19.000	15.10	3.59
9 D..	Cu., 91.5 ; Al., 7.75 ; Si., 0.75 .	1.900	53,000	19,000	6.20	15.50
10 D..	Cu., 90 ; Al., 9 ; Si., 1.........	1.890	69,930	33,000	1.33	8.30
11 D..	Cu., 63 ; Zn., 33.33 ; Al., 3.33 ; Si., 0.33	1.900	70,400	55,000	0.40	4.33
13 D..	Cu., 92 ; Al., 7.5 ; Si., 0.5.......	1.930	46,550	17,000	7.80	19.19

These are all familiar aluminium bronzes, with minute doses of silicon, with the exception of No. 11, which is a brass of nearly the proportions of Muntz metal, to which a small percentage of aluminium has been added, replacing the tin of the writer's "maximum" alloy with equal apparent effect. With this one exception the alloys having more than 60,000 lbs. tenacity are decidedly deficient in ductility, and those having a tenacity exceeding 70,000 lbs. are probably too brittle for general use in construction.

Magnesium, although still a rare metal and somewhat costly, has always seemed to the writer a possible rival of the more common metals for light construction. It is lighter than aluminium, its specific gravity being 1.74 (108 lbs. per cubic foot), but its tenacity is only 22,000 to 32,000 lbs. to the square inch ; so that it would sustain from 28,000 to 42,000 lineal feet of its own substance. It seems more likely, therefore, as in the case with aluminium, to prove more serviceable in alloys than pure ; but although the writer has been experimenting with the metal for about 30 years, he does not know of any of its alloys which would be valuable for aeronautic constructive purposes.

Manganese, chromium, nickel and silicium have also found application in the arts as alloys both in bronzes and in some varieties of steel. They impart to the latter various qualities, such as increased toughness, hardness, resistance to penetration, etc., but without very materially increasing its tenacity ; so that, for a beginning at least, they cannot be said to be

* Appleton's "Cyclopædia of Applied Mechanics," Supp. (Modern Mechanism), 1892, art. Alloys, p. 24.

superior to carbon as an alloy for steel intended for aeronautic constructions.

Mark on Specimen.	Dimensions of Specimen.		Test.			
	Size.	Length.	Tens. Str.	Elas. Lim.	Per cent. Ex.	Per cent. Cont.
21..........	500	2	127,805	107,640	19.00	54.20
19..........	500	2	120,670	107,380	17.75	55.57
29..........	500	2	124,760	106,360	18.50	52.46
40..........	500	2	124,250	106,360	18.25	57.30
			Specification			
			120,000	100,000	15.0	50.0
66..........	500	2	140,050	119,170	14.75	51.27
49..........	500	2	140,000	118,700	15.00	46.90
68..........	500	2	139,080	118,630	15.25	47.65
77..........	500	2	140,050	118,150	14.00	49.57
14..........	500	2	140,760	117,990	15 00	50.40
55..........	500	2	137,540	116,580	18.75	48.23
35..........	500	2	138,520	115,100	14.75	47.57
16..........	500	2	133,430	114,080	16.00	51.82
			Specification			
			130,000	110,000	12.0	40.0
24..........	500	2	181,190	146,300	9.25	33.2
60..........	500	2	155 430	134,990	12.00	39.5
15..........	500	2	148,780	134,480	12.75	45.0
26..........	500	2	154,500	126,175	11.50	41.5
23..........	500	2	143,730	122,690	12.50	44.8
28..........	500	2	148,780	120,670	11.50	44.4
25..........	500	2	141,630	120,670	14.0	47.1
7..........	500	2	140,100	120,670	11.60	40.4
			Specification			
			140,000	120,000	10.0	30.0

As an illustration of what can be done by specially skillful and intelligent treatment of some of these alloys, now called steel, the above are presented. The composition is not given, as the metal is at present not publicly described; but it is stated by the maker that these qualities may be regularly and certainly secured at any time, and the specification as readily met as is customary with the steels now put on the market by the most successful makers.

The maker of these steels would employ them under variable loads not exceeding 40,000 lbs. per square inch, under steady tensional or compressive stresses of 75,000 lbs. maximum, and in short compression members under loads of, for the strongest of them, as high as 125,000. The samples here described were all cut from round rolled bars turned down from ¾ to ½ in. in diameter. Made into a railway axle, and tested by the standard drop test, the deflection was one-half that usual with the standard steels, and it broke under about 40 blows. Its elastic

limit was found at 84,000 lbs. per square inch. The specification entered in the table is that which may be filled safely with each quality. As the ratio of tenacity to weight per cubic foot is, in the strongest of these last compositions, above 300, they are available for aeronautic constructions.

CONCLUSIONS from what has preceded seem very clear and unquestionable as far as the most important matter in hand is concerned. Steel is, for general use, the most promising of all materials yet known.

For the motor machinery of any system in which lightness is a primary object and strength even more essential than in ordinary construction, the form of the machine must, first of all, be such as involves the employment of tension and compression members as exclusively as practicable, and the entire elimination of every unessential beam or girder; this being the way to secure the highest effectiveness of whatever materials are available. It is usually possible to construct designs in which no other parts than shafts and pins shall be subject to cross-breaking stresses. Frames and running parts may be made of steel; struts and ties of the best forms for strength combined with minimum sections and volumes. Even irregular parts, such as steam cylinders, may sometimes be built up of malleable materials of maximum value, constructively. Drop-forging and castings of steel as high in carbon or other hardening and strengthening elements as is consistent with needed malleability and ductility are better than even any form of aluminium alloy yet discovered, probably better than any alloy of magnesium, where irregular and unmalleable shapes are demanded. Fine steel wires and ribbons having a tenacity of 300,000 lbs. or more per square inch, and thin steel tubes of nearly equal strength, represent the highest result yet attained in uniting the two essential properties.

Among the possibilities we are apparently to look for improvement by the introduction of manganese and nickel, although we can as yet see nothing very promising in either direction. Of the two, nickel appears to offer the best results. We can see nothing of value among the alloys of the familiar metals—copper, tin and zinc. We know what is the character of the best possible combination, and that no alloy of these metals is possible possessing unknown properties. We have detected the "maximum alloy," and have learned its properties. It cannot compete with steel in the principal parts of such machinery as we have in view.

Of the rarer metals, aluminium has been expected to give a great advantage; but we find it far inferior, both in itself and in all its known alloys, to even common steels. Magnesium, a still more promising but much less well-studied metal, gives evident promise of competing successfully with aluminium, both in lightness and strength and in the combination of the two qualities, for such machinery; but we have, at least as yet, no proof that it can in any manner ever compete with the fine steels. It opens a field for new investigations; but it is

a possibility rather than an expectation of advantage that attracts us to its exploration.

The fibrous and textile materials do not seem to promise usefulness for our present purpose; all, even including silk, are inferior to metal of similar weight and dimensions, in combining strength and lightness. No substance in nature, so far as we to-day are aware, is the rival of the best steels of the day, and these steels are being every day improved.

For the hull of the air-ship, and for other vessels requiring similar properties, it seems very possible that some such substance as the paper employed for racing boats, perhaps even some of the aluminium and other alloys, or those of magnesium perhaps still more possibly if not probably—substances which combine a certain stiffness and substantiality with lightness, and adapt themselves especially to the production of such forms of construction, may answer the purpose. This is quite a different matter, and time and trial only can decisively settle this question. It would at the moment, however, seem probable that steel in thin sheets will prove unrivalled for even such parts as these, and the gist of the whole matter may be summed up in the statement that steel, in its various known grades and qualities, is to-day the one unrivalled substance for all constructions, and that the problem of the moment is the finding of simple and cheap methods of forming of that metal the parts and shapes desired without loss of its wonderful combination of strength, ductility, resilience and comparative lightness, as illustrated by the best qualities known, and at present only in small sections. That is to say, we have yet to learn how to secure the real, the intrinsic—if I may use that word—the intrinsic strength and resilience of the metal in all forms and in whatever mass may be demanded.

The reduction in weight of machinery and structures, other things being equal, will be very nearly proportional to the strength of the material, and every increase of that strength gives corresponding increase of available power for a stated weight of machine or part. The machinery of the fastest torpedo boats and steam yachts is constructed of metals having a tenacity of, as an average, probably about 50,000 or 60,000 lbs. per square inch. Could metal of similar density be given, as now seems likely, twice and three times that strength, or, even as is apparently not impossible (judging from our present knowledge of the subject), five times those values, it would become practicable to reduce the weights of our aeronautic machinery and of steam-engines for marine purposes from about 50 or 60 lbs., the present minimum for such cases, to not above 10 or 15 lbs. per H.P., and without in any manner reducing the factor of safety or adopting a more dangerous speed of piston or of rotation, or otherwise departing from that system of construction to which we have been feeling our way slowly and steadily for a hundred years. The inherent, natural, maximum, molecular strength of steel of finest quality is certainly not less than 400,000 lbs. per square inch, that being somewhere about the limit which we approach by refine-

ment and wire-drawing or other method of working, as illustrated in finest wire and thinnest sheet and ribbon. There seems no reason to doubt that we may hope, at some time, to secure this limiting strength in large masses; if thus successful we shall be able to build safe and strong engines of 5 lbs. weight per H.P. or less.

The fields remaining open for further research are, as always, dimly revealed and of unknown extent. It is only of the copper-tin-zinc combinations that we can say that we know their limitations. It remains to be ascertained whether iron and steel are capable of still further and more wonderful improvement; whether they can be given maximum value in all forms and sizes of parts; and whether they are capable of alloying with new or old "useful metals" with advantage. We have still to seek the best of the rarer metals and their best treatment and combinations. It is even possible that we have before us the contingency of finding ways of producing organic materials, or chemical compounds of non-metallic substances, that shall rival and excel not only silk and hemp, but even steel in the qualities which we here discuss.

Meantime, whatever the outcome of our as yet incomplete or unattempted researches in these fields, we have the privilege of asserting that, even with known materials and with known methods of construction of familiar designs, the problem of motive machinery is practically solved, and that we can to-day build motors of steel that excel those of nature, whether of fish, beast, or bird, in their combined power, lightness, and compactness. The problem of aviation even is no longer one of weight and power of motor, although it would be folly to assert that there is not still much to be done in that direction. Should it prove ultimately possible to construct the air-ship and its other accessories, we may now feel sure that it will not be that hitherto apparently greatest of all visible obstacles, the prodigious weight of motor machinery as compared with that of the birds, which will impede progress. The problem of the hour is now, for aeronaut and aviator alike, that of the construction and especially of the management of the hull and of the propelling wings or screws of the floating or self-supported air-ship.

DISCUSSION.

Professor Johnson, of St. Louis: I had the pleasure of reading in advance Professor Thurston's paper. I think it is a valuable treatise on the subject of the strength of materials with reference to their weight. I found in the paper many things which I believe are new and which I believe will be found by others to be new; it is therefore of great interest to constructors. I have no doubt the paper will be found valuable on the subject of strength of materials with regard to constructions generally.

I wish to say that on the subject of the strength of timber we unfortunately seem to know the least, although it is the commonest building material in use; but from tests which are

now going on under my own direction I have gathered facts that bear directly on this business, and the result indicates that timber is likely to be invaluable as an aeronautical building agent. Timber that weighs one-twelfth as much as steel proves to have a tensile strength of 20,000 to 30,000 lbs. per square inch, and when you multiply that by 12 you have pretty strong steel; moreover, it is as yet a question, I believe, whether steel can be made to hold this high value of strength when made in larger sizes than when used in small specimens, whereas timber has the same strength in large sticks. I think ordinary constructors do not make a mistake in using timber in place of steel in many places. I think select timber from some of our best species will perhaps give the strongest and without doubt the most elastic of any material that is now known for aeronautical constructions.

Mr. Myers, Frankfort, N. Y.: In addition to what the gentleman has said, I will state, I have used it for many aeronautical purposes, and it has seemed to serve my purpose better than the metals, particularly as it filled the requirement for less money than any other material I have used.

Mr. Landreth, of Vanderbilt University: I would like to call attention to a point not brought out by Mr. Johnson. Lightness is not the only thing to be provided; for we must not only provide for meeting the stresses and for reducing weight, but we must offer as small a cross-section to the moving air as possible in addition to having lightness of weight. Now while the timber might carry a higher load per unit of weight than steel, it will present about three to three and a half times the cross-section, and that would produce a very serious obstacle, while otherwise meeting one requirement that was most desirable.

Professor Christy, of the University of California: I would like to ask Professor Thurston about the strength of the copper aluminium alloy.

Professor Thurston: The aluminium bronze is stronger than the ordinary bronze, and in some compositions it is considerably stronger, but not enough so to bring it into consideration as the material to be used for a machine in parts subject to special stress; it comes in use for some bearing parts, but it is not sufficiently strong for general construction. As far as aluminium, there is no promise in that direction. We can get more resistance from the same weight of steel.

FLYING DEVICES.

By George Crosland Taylor, F.R.G.S., A.I.E.E., Helsby, Cheshire, England.

PART I.—MATERIALS FOR AERONAUTICAL CONSTRUCTION.

It is only within the last few years that materials suitable for aerial constructions have arrived at any degree of perfec-

tion; the time was, say, not 20 years ago, when such materials as cane, bamboo, wood, paper, oiled silk, varnished textile fabrics, and such similar common materials were the only ones available for experiments; now various inventions have brought before the general engineering world, as articles of every-day use, different sections of tubes, plates, wire-woven webs, etc., of more or less better understood metals, such as aluminium and its alloys, nickel and steel and their alloys; also vulcanized fibers which stand the damp, celluloid, a transparent flexible material, and also more durable elastic varnishes, such as alboline, used for transparent wire woven roofing, as used on the roof of Westminster Aquarium, London, also new kinds of American and Australian woods; india-rubber compounds are also better understood, and are now made less liable to deterioration.

In constructing a flying machine, whether for free flight or for more or less suspended aerial planes, for trying experiments, or for estimating the lifting forces of the wind, it is a great mistake to try to reduce the weight too much.

If too light the strength of the structure is sure to suffer, and, moreover, the control which is to be exercised over the wind is practically gone when a machine structure is too light. When a bird is flying it is really creating for itself a gale of wind by its own speed, and the consequence is that a structure of large area and of light weight is easily tossed about by the wind, it has no momentum of its own, having no weight to store its momentum in, like an engine with no fly-wheel, and it is therefore, as a rule, unsteady.

Aluminium pure and simple, used as a constructive metal, is not suitable for parts which have to bear a working live strain, such as struts, stays, levers, etc., or for the material of the planes or wings in the form of a sheet; and for this reason: the metal is soft and liable to tear in the sheet. As the lightest commercial metal known, the temptation is to use it, but the thickness must be increased to get the same strength as steel for tubes, bars, etc., and this quite discounts its extra lightness; moreover, the price rises considerably, if compared with steel, when drawn into bars, wire of various sections, etc., but for bearing surfaces, valves, cylinders for aerial engines, and other parts not taking a live strain it may be adapted.

The best way to use aluminium is in the form of a wire-woven web, the holes in which are closed by an elastic varnish; this makes the lightest form known of metallic sheeting for aerial planes, but it is very expensive and cannot (in England) be woven at the present time over 8 ft. wide, although there is no doubt that a machine could be designed so as to weave it 12 ft. wide.

Wire of this description can be obtained .007 in. diameter, and the finished web weighs from 1¼ to 4 oz. per square foot, according to the thickness and number of ends per inch, and whether it is made of *aluminium*, *brass*, *phosphor bronze*, or *iron wires*, the web when varnished is permanent, and holds

water from passing into it or through it for an indefinite period.

The usual kind of *transparent wire-woven roofing* weighs 0.5 lbs. per square foot when about $\frac{1}{16}$ in. thick. This may answer for some aerial planes, but it is altogether too heavy (this size) on account of the thickness; it is very strong. The finer and lighter kind, as pointed out by the writer, is admirably adapted for the construction of aerial planes.

Very fine *phosphor bronze wire-woven web*, such as used for dynamo brushes in England, weighs 4 oz. per square foot, and could be made half the weight if not so closely woven.

Very fine *iron wire gauze web*, used for sieves, weighs 3 oz. per square foot, and if half as closely woven would weigh 1½ oz. per square foot. This is about as light as it is made. If aluminium wire .007 in. diameter were substituted for the iron wire, it would weigh two and one-half times less, or .6 of an ounce per square foot, or with the varnish on about .75 oz. per square foot.

The price of the aluminium wire is some 25*s*. per pound; hand woven costs perhaps 2*s*. per foot, 3 ft. wide, and the varnish 15*s*. per gallon, for about the usual weight of linseed varnishes containing a siccative. Perhaps, as a rule, the varnish adds from 15 to 30 per cent. to the weight of the wire web.

It is most important that such a substance as varnish or a similar waterproof coating should be used, as in case rain comes down the weight of the fabric is increased for no purpose. The transparency of the varnish enables any one to perceive any damage at once.

The application of varnish of a permanent nature to metallic wire-woven webs for roofing purposes is not new, but the writer claims novelty in the application of the system to aerial planes, or to wing fabrics for flying purposes, and there is no doubt that its application may prove more useful than textile or rubber applications for a similar purpose.

Vulcanized fiber .035 in. thick: a piece 6 ft. by 3 ft. 6 in. weighs 5 lbs., or 0.238 lbs. per square foot.

Vulcanized fiber .028 in. thick: a piece 5½ ft. by 3½ ft. weighs 3¼ lbs., or 0.174 lbs. per square foot. This fiber, made in the United States and in England, is colored red, black, and white; it is principally used in the electrical and hydraulic industries. Wet and damp barely affect the better qualities; when dry, it is tough and strong, takes a thread and holds screws well, can be molded, and may be bent to shape in very hot water.

Ebonite Sheet.—A sheet 3 ft. 6 in. by 1 ft. 6 in. by 18 S. W. gauge weighs 17 oz., or 0.283 lbs., per square foot. This is very useful for small experiments with aerial planes or screws, for it can be bent or molded in boiling water, and when cold sets and keeps its shape.

Brown canvas backed packing paper, about 24 S. W. gauge, weighs 8 oz. for 12 sq. ft., or ⅔ of an ounce per square foot. This is very useful for plane experiments, as it does not easily

tear. The canvas is of the lightest description, and will not separate unless wet.

Oiled Canvas, Linen, etc.—A piece 16 in. by 5.75 in. weighs $1\frac{2}{3}$ oz., or 2.6 oz. per square foot. As a rule it is full of small holes, but these are of no consequence; it is very pliable, and runs about 22 S. W. gauge; for small experiments it is useful, and may be relied on in wet weather and high winds.

Various canvases, oiled silks, etc., have been tried by me, but with the exception of the Willesden patent waterproof canvas, as used for tents for the British Army, it is not advisable to employ them, as they are all liable to rot if remaining damp when not in use.

The *Willesden strong canvas*, 53 in. wide, costs about 2*s*. per yard and upward.

Another very light textile fabric is what is known as *proof silk*, which is thinly spread with a vulcanized rubber compound; it is airproof and waterproof, and weighs .34 of an ounce per square foot, costs about $2\frac{1}{4}$*d*. per square foot, and can be obtained in long rolls about 5 ft. wide; a heavier sort is made in cotton sheeting, and costs about 2*s*. per pound. The writer uses it for kites.

Sheet celluloid (cotton and camphor) is transparent, easily molded in boiling water, and can be obtained in rolled sheets $\frac{1}{32}$ of an inch thick; these sheets measure 36 in. by 20 in., and cost 4*s*. 9*d*. per pound. They are useful for small winged models, will stand the wet, but in time shrink and become brittle. Made in America.

The supporting frames of aerial apparatus are best made of *steel tubes* or other rolled sections. The finest thin steel tubes are made in France, and the larger sizes in England. The French make them of all sections (hollow), such as ovals, hexagon, star-shaped, etc., and of accurate thickness throughout. The prices vary from 18*s*. per pound for the thinnest sections of tubes (used for surgical apparatus) to 2*s*. per pound for larger and thicker sizes.

Taper and *bent tubes*, generally brazed or drawn, are made (in England) of all variety of shapes by special makers for the bicycle trade, being of steel of very superior quality and all bright finish.

Umbrella section steel makes a good support for light planes. A section $\frac{5}{32}$ in. by $\frac{1}{8}$ in. deep, 6 ft. long, weighs 2.1 oz., and a section $\frac{3}{32}$ in. across the base by $\frac{1}{8}$ in. deep, 6 ft. long, weighs 1.95 oz. There are smaller sections made, but the length of 6 ft. is not exceeded on account of the size of the tempering furnaces, and there is a liability (particularly for the smaller sections) to twist in the process.

For the metal parts of aerial engines and for moving or rubbing parts, the 5 per cent., $7\frac{1}{2}$ per cent., 10 per cent *aluminium bronze alloys* appear to be the best, and the aluminium lightens the parts a little. It requires care and some experience to cast, but the rolled bars, sheets, and wire can be obtained from the various aluminium manufacturers to order.

These alloys the writer finds to be much superior to any of

the bell, gun-metals, or phosphor-bronze, and tin alloys; the castings being stronger and free from blowholes, they can be made lighter than the latter alloys, and do not easily tarnish. The wearing qualities are most excellent in the higher grade aluminium bronze alloys. They are to be preferred as a substitute for copper sheets in aerial boilers, and can be thinner for similar pressures. The 7½ per cent. and 10 per cent. cannot be soldered, although possibly some method may be found that will be satisfactory.

In constructing aerial apparatus it is necessary not to interrupt the continuity of any spar, rib, tube, etc., belonging to the main frame, by drilling holes or making thinned-down holding places; it is better to clamp around them, and it is a good practice, while preserving a more or less rigid framework, to bed pieces of thin sheet rubber, of the best quality, around the tubes, etc., between tube and the clamping part; it makes a much better holdfast, stops the parts from slipping, deadens shocks, and imparts elasticity to the framework.

With regard to the weight of the planes or wings, it should be clearly understood that the apparatus is to be carried by the air when in motion, and practically has no weight to be lifted up and down relatively to the power employed, and that unless there is some strength in the structure it soon comes to grief.

The weight of the apparatus should be centered below the planes or wings, and these should be spread in an horizontal inclined manner, either sideways or backward, in a manner similar to Mr. Lawrence Hargraves' steam and compressed-air aerial flight machines; but perhaps the boiler could be poised vertically instead of horizontally.

A really practical flying ship ought to be capable of raising itself vertically from the ground, and then to propel itself forward. It should not require a preliminary run, or it will always be troublesome to get a start. This requirement will, in the writer's opinion, perhaps be attained eventually by means of properly shaped revolving screw propellers.

PART II.—OBSERVATIONS AND EXPERIMENTS.

From many observations on land and sea as to the flight of birds, it may be assumed that land birds which do not habitually fly very high do not soar, as do sea birds, which have long distances and stronger breezes to face. The contours of the land surfaces affect the wind currents, deflecting them often up and down, while, on the other hand, sea breezes are straight and parallel with the surface of the water; they furnish a more even sustained pressure, thereby making it easier to soar.

In order to ascend to a great height, birds will be often seen to locate themselves in the air over the summit of a hill against which a wind is blowing, and then, without apparent effort of wing, to rise vertically, being actually pushed upward by the wind, which is deflected more or less vertically. A hill

with a cliff at the top, such as many in Cheshire, is the best for this purpose. A hill some 500 ft. high will aid the soaring bird to rise at least 1,500 ft., from which elevation an inclined downward glide (in the case of a seagull) will carry it at least 3 to 3½ miles without flapping.

Birds may be approximately divided into two classes: the long and narrow winged, such as the eagle, hawk, swallow, swift, and many of the sea birds, such as the gull or the albatross; the other division have short broad wings, such as the partridge, pheasant, sparrow, thrush, etc. Of course there are intermediate varieties, such as the wild duck, plover, pigeon, jay, snipe, magpie, etc., capable of many styles of flight.

The larger the bird and the longer its wing, the slower are the strokes of the wing, and the experiments with small planes on a whirling table by Professor Langley, in the United States, all tend to prove this advantage of the narrow wing, judging by the pressures obtained; moreover, flight at a great speed is easier than slow flight, the air acting as a better sustainer, because more weight of air is displaced or passed over in a given period of time.

Thus the albatross gives 10 beats per minute, the pigeon 170, and the English swan 120 beats per minute.

As to the speed of flight, it varies very much. Perhaps the pigeon is the best known, and records of flying matches of carrier pigeons, on a distance of, say, 200 to 300 miles, vary from 750 to 1,025 yards per minute. In England it is usual to "rubber stamp" the under side of the wing with the owner's name and address, and to affix a small loose-fitting metal ring stamped with a number on one of the legs.

The speed of the common wild duck, as observed on the river Dee estuary at Chester, is about 40 miles per hour. Such birds have been observed to face winds of quite 60 miles per hour, by rising vertically against it, and then gliding down forward, thus progressing by a series of vertical tacks against the wind.

The writer has observed an albatross close to a ship glide along for hours, at 14 knots, in a fair gale of wind, without any wing movement except a slight rolling motion of the body, and he once observed such a bird asleep for some time on the wing, its head being stowed over its back between the outstretched wings. There is no record of speed of the albatross on long distances, but there is no doubt that the speed attained may be anything up to 50 miles per hour under ordinary circumstances.

One caught by means of fish-hook and bait measured 14 ft. from tip to tip of wing, and the weight was 18 lbs. In drawing them in by a line the birds soar up like a kite, and there is a considerable tension on the line.

This may be repeated with a swallow, which alive weighs one ounce. If killed by an anæsthetic, with its wings stretched out, tail and head in position of flight, it will when cold retain this position. The next thing to do is to mummify it by drying it in an atmosphere of carbolic acid vapor. When once

dried it will keep indefinitely if kept dry. This dead bird, which now weighs only ¼ oz., should then have a thin thread fastened to its beak, and if then hung from a pole 20 ft. high (or higher) it will, in a moderate wind, soar up 20 ft. high, none of the weight of the bird being left on the thread, which only serves now to keep the swallow head-on to the wind, whatever slight tension there may be on the thread being produced by the wind pressure on the bird, and amounting to no more than 7½ per cent. of the weight of the bird. If a weight of 1¼ oz. be next added on the breast of the bird, it will still ascend, but not so quickly as before. The illusion thus produced of bird flight is so complete and real that on one occasion several swallows circled round and round it, calling and twittering to it for a few minutes.

The wings of a wild duck measuring 2 ft. 6 in. from tip to tip and weighing 2½ lbs., or at the rate of 1 sq. ft. of wing to 2½ lbs., were attached to a rough gutta-percha figure of a man, 12½ in. high; the whole weighed ¾ lb. The wings were only about three-quarters open. This arrangement, attached to a pole by a string, soared up from the ground easily in a moderate gale with 1¼ lbs. additional attached. There being no instrument to show the speed of the wind on the instant, in the writer's possession, it is impossible to state correctly its velocity, but it is very probable that the speed of the wind was 25 miles per hour. This shows a large margin of lifting power. Other experiments are described further on.

There is more or less surplus margin in the carrying powers of birds. A rook some 300 ft. high in full flight, once dropped a potato weighing 8 oz. at the writer's feet. Various birds of prey, such as eagles, vultures, hawks, carry up to their nests all kinds of animals, including and up to the size of lambs.

Powerful birds like the hawk, which is a most graceful flyer, can rise in the air vertically, poising with head up and tail down, but this flight seldom exceeds 200 ft. in height, when they turn upon their wings and pursue a horizontal course.

The writer's theory regarding the soaring and sailing of birds may here be advanced.

Any ordinary metallic body—say, a ball of iron—falls 16.1 ft. per second in the air. If it weighed 1 lb. then it would develop 16.1 foot-pounds per second, and this tendency to fall is stored up in the ball, owing to the force of gravity, unless prevented at the first moment. If prevented, then it is only the weight of 1 lb. Now a bird is always ready to fall, but not in the same increasing ratio as a metal ball, because it is not so dense, volume for volume, and because the weight is spread over greater space or surface. Very probably the specific gravity of a bird is less than water, and therefore seven or eight times lighter in proportion than iron; therefore the tendency will be to develop less foot-pounds in one second, owing to the lighter specific gravity and to the increased resistance or volume of air.

The writer's theory of gliding flight is this: That in order to maintain the equilibrium of a bird in the air, neither rising

nor falling, a volume of air per second, equal in weight to the foot pounds per second capable of being developed by the falling bird in one second, should pass under the wings and body of the bird in order to maintain the poise ; more volume of air than this produces a tendency to glide up, because the weight of the bird and the screw-like propulsion of its wings* prevent its being blown backward except in a gale ; the soaring of large birds being generally performed against a wind, and this wind passing a great weight of air under their wings.

In sailing against the wind the sustaining effect of the volume of air passed over is nicely adjusted by the inclination of the wings from the horizontal position ; thus the more inclined —say, up to 35 per cent.—the more the lifting force at a given speed, and with lesser angles there is less tendency to rise on the wind.

Thus the effect of the volume of an horizontal or a vertical column of air can be made use of by the bird to rise, to stay

Fig. A.

on level flight, or to fall, by simply varying the inclination or angle of the wings, and of the speed of soaring flight, or in flapping flight, by variation in rapidity and arc of the wing stroke.

A very tired pigeon will move its wings very slowly, but with larger stroke, sometimes so much so that the wing tips meet together and beat loudly and visibly over the back at each stroke.

In order to maintain easy command of the air, it seems evident that the volume of air passed over by birds, which is equivalent to their displacement, must be much in excess of that which is actually required for straightforward level flight at ordinary speeds.

It is very doubtful whether the theory that a partial vacuum

* This is really a theory of active displacement, as against passive displacement of air, as is the case when a balloon floats in air.—G. C. T.

is created on top of the wing as it advances forward has really any value at all, or even that this vacuum exists to any useful extent.

The wings of a bird are really like blades of a screw and act as such, the front of the wing being inclined upward on the up-stroke and downward on the down-stroke.

Take, say, two left-hand wings of a bird, and mount them on a piece of gutta-percha, and place this on a steel spindle, as in fig. A.

This forms a perfect screw, and if held in the wind it revolves swiftly and easily in the direction of a clock's hands if the wind impinges on to the top side of the wings, and in the reverse direction if the wind is directed against the under side. Fig. B shows the screw of fig. A from the other side.

Fig. B.

It is very probable that if this screw revolves at a given rate with a given speed of wind, it will be found that the similar wings on a bird have to move at the same speed at the tips as in the screw form (in a similar speed of wind) in order to attain flight.

This is only a theory, but it is a fact that this natural-wing screw, when rotated by a band from a wheel at a fair speed, tried to rise up out of the bearings, and that it offers considerable resistance, as it lays hold of the air in a very efficient way for its size. It is 18 in. across from tip to tip, or 20 in. across measured on the curve. The area of the screw all over is 60 sq. in.; the weight of the wings, spindle, and balance-wheel is 11.75 oz.

It should be borne in mind that 1 cub. ft. of air weighs about 1¼ ounces, and this is some weight to lay hold of quickly.

Birds which can soar and glide do not get so fatigued as those which take short more or less erratic flights. There is little or no muscular exertion in soaring, advantage being taken by the bird of the force actually existing in the wind sailed against.

From observations made of seagulls in flapping flight, the writer feels sure that on the down-stroke the body of the

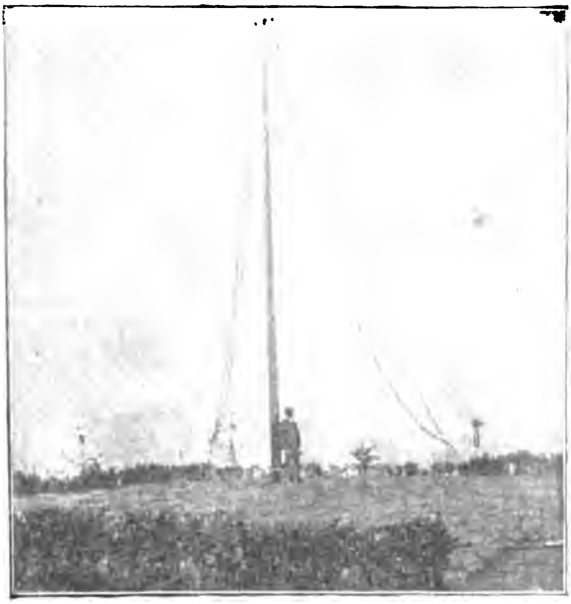


Fig. C.

birds rises a little and on the up-stroke it falls, oscillating, as it were, above and below an imaginary straight line of flight, whereas in gliding flight the body remains still, relative to the wings, and just below them rather than on a plane with the wing surfaces.

From experiments with wing-like models suspended as a kite from a pole 35 ft. high, the writer concludes that the power to be exerted on the wings to sustain the body of the bird (head-on to the wind blowing against it, or through the wind created by the flight in a calm) is much less than is commonly supposed; it is perhaps not more than 7½ per cent. of

the bird's own weight, which is to be exerted as producing power on the wings at each stroke in order to keep up against the wind, or, if there be no wind, to keep up an artificial wind of their own.

The writer's experiments on gliding or soaring devices have been made with a view to ascertain the lifting power of wings as produced by a current of wind against them, which is assumed to be equivalent to the inverse action of the bird progressing against the air in flight.

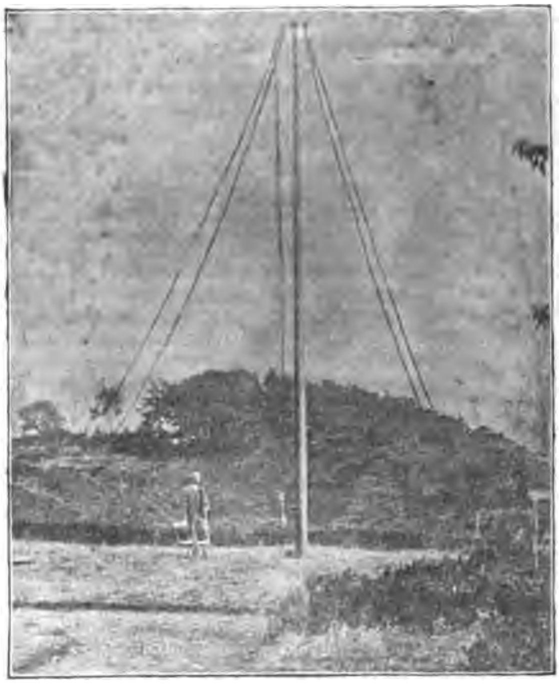


Fig. D.

The following two experiments were made by means of a tall strong pole, some 35 ft. high, 4 in. at the top and 13 in. diameter at the bottom. To the top of this pole two halliards were fixed with pulleys and ropes, revolving on a spike 1½ in. diameter, the whole construction being practically rigid.

The pole (see fig. C, looking west toward the ocean, and fig. D, looking south, showing the declivity of the hill) stands on an eminence 220 ft. above the sea-level, and from the foot

Fig. 1.

of this hill, immediately below, low-lying flat meadows stretch away to the Mersey estuary, thus commanding an open view, with no impediment to the wind, for 20 miles right to the ocean.

Fig. 2.

The experiments were conducted in two ways—viz., by suspension of models on a string from one of the pulleys at the top, and by vertical ascension without suspension—that is to say, the apparatus was to run vertically up on a stretched

string used as a guide only and not as a suspender, and was to rise to a height of 5 ft. from the top of the pole, or a total lift vertically of 30 ft.

Figs. 1 and 2 display an ordinary wooden doll 17 in. high (sold in England for 25 cents), with jointed limbs.

Fitted at the sides with strong binding screws of brass, and attached thereto, is the pair of wings, in a position rather to the rear of the figure. Each wing measures in its greatest length 20 in. and in its greatest breadth 9 in., giving a total area for each wing of 110 sq. in. Across from tip to tip it is 3 ft. The weight of the doll and wings is 2¼ lbs.

Fig. 3.

The wings are made of transparent celluloid a little over .065 of an inch thick, and are bound by metal rivets to a strong No. 10 S. W. gauge steel wire, the wing being bent somewhat into the screw-like form of a bird's wing. On the forehead of the doll, projecting 1.75 in., is a metal staple shown better in side view, fig. 3.

A rear view of the model is shown in fig. 4. The string attached to the head is simply for the purpose of holding it up to be photographed.

If suspended from the pole by a string attached to the breast at the point shown by the dot on the chest of fig. 1, the model floats up into the air (when there is a favorable wind) perhaps

15 to 20 ft.; but unless there is a very steady wind it rocks about and performs various tricks of turning round and round and upside down. If there is a wind of about 20 miles per hour it becomes unmanageable.

If the string be changed to the wire staple in the forehead (fig. 3), so as to bring the holding force upward and forward, the result is perfect; however unsteady or strong the wind may be, even in a gale, the figure ascends to over 30 ft. from the ground and there remains poised, the figure being upright, as shown in fig. 3.

The wind never allows the model to swing down against the pole. Even if the wind suddenly lulls while the model is at a great height, the pressure of the air under the wings, resulting from the sudden curved descent, rarely fails to bring it gently to rest before it reaches the pole. The model can be left out of doors for many weeks without harm. Sometimes

FIG. 4.

it has been aloft for over a week, a source of great interest to many observers.

A spring balance inserted between the string and the doll shows the varying pressure of the wind on the model, the weight of the doll, and the strain on the string.

In a calm, when the winged doll hangs at the base of the pole, it shows 2¼ lbs. to be the weight of the model, while in a wind, at 5 ft from the ground, the wind pressure on the wings, the thrust and the weight of the model combined mark 3¼ lbs. As the model soars up higher this record gradually becomes less, until at 25 to 30 ft. elevation it only shows 3 oz., the weight of model being completely borne upon the wings, and the string more or less slack.

This pressure of 3 oz. is therefore the resistance to be overcome continually by reason of the wind's blowing the model backward, and therefore is the actual horizontal component of wind pressure against the wings and the doll's body. This body is vertical, and its area is about 30 sq. in., more or less rounded.

This horizontal component of wind pressure amounts to just 7½ per cent. of the entire weight, and in winds blowing over 25 miles per hour a weight of 1 lb. can be attached to the model without any inconvenience, although, perhaps, it may ascend not quite so quickly.

With an arrangement of wire loops in front of the figure (not shown in the engravings), so as to enable it to slide up on a vertical string to the top of the pole, a strong wind lifts it up vertically 30 ft. to the top, showing and proving the fact that once fairly up, as on the suspended string experiment first described, the weight of the doll actually rests on the wings, and the wings on the air blowing under them.

Another experiment has been tried with the vertical string arrangement. A pair of wild duck wings, of ordinary full-grown size, was affixed by means of steel wires to a wooden slotted frame, as shown in fig. 5. Through this frame the

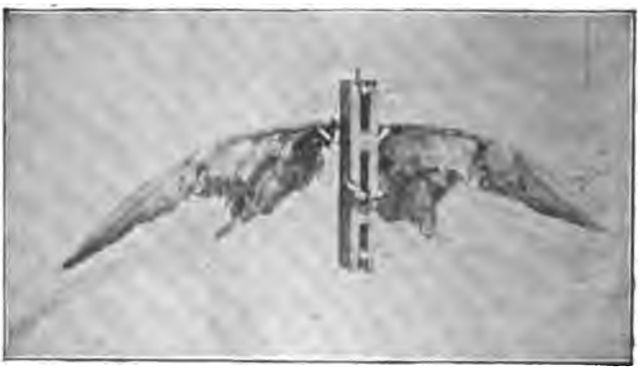


Fig. 5.

string passes, and runs against guide wheels fixed inside of the frame.

This apparatus weighs 7.5 oz., measures 22 in. from tip to tip, and about 75 sq. in. in wing surface (the wings not being fully open). In a fair wind of about 20 miles per hour it rushes up the 30 ft. vertical string guide in two or three seconds, and continues to rush up and down as the wind varies in velocity. If the wind is steady it remains at the top. Sometimes it has done so for an hour at a time, looking not unlike a hawk. Having no apparatus to measure the pressure of the wind horizontally against the wings, which are inclined at a small angle, there are no measurements to be given of this performance, but it seems very probable (on account of the small vertical surface exposed) that the tension will not exceed 6 per cent. of the total weight. If the apparatus falls vertically more than 10 ft. at once, it is very seldom that the

pressure due to the fall can arrest the gathered momentum, unless the wings are very equal in size and accurately coincide as to relative position. Either in this or any other model they spin round like a top.

These experiments with models suspended on a string are thought to be instructive, with regard to probable safe means of enabling a man to try all sorts of wings, and actually to ascend and to poise himself on them, while at the same time remaining in a position of reasonable security from personal injury. Particularly because if he were suspended from a cross girder, and there was no pole to fall against, then if one wing broke he would simply oscillate about until rescued. Apparatus to test this is in course of preparation.

Further experiments for securing equilibrium have been and are being made with tailless kites, and thus far no models of any kind have been injured by dashing down against the pole, showing the safety of the system.

ATMOSPHERIC GUSTS AND THEIR RELATION TO FLIGHT.

By A. F. Zahm.

In the year 1882 I read before the Scientific Association of Nôtre Dame University a paper purporting to maintain the feasibility of soaring in a horizontal wind of uniform velocity and direction; but in the discussion which ensued my brother, Rev. J. A. Zahm, Professor of Physics in the University, pointed out to me that soaring in a uniform horizontal current is equivalent to soaring in a calm, which means no less than perpetual motion or the creation of energy. I saw then that flight on rigid wings must require a wind having either an upward trend, or a variable velocity, or a variable direction. As it appeared evident that such an upward trend could not prevail so generally over the earth as the flight of vultures and gulls would demand, I concluded that these must derive their motive power largely from the pulsations of the wind or from its variations of velocity and direction. The assumption then made I have ever since regarded as unassailable, and it has seemed to me very desirable to learn from extensive measurement the actual nature of such pulsations.

Finding some leisure in November of 1892, I began observations on the behavior of the wind by mounting a universal weather-vane or anemoscope in a large field quite level and clear of trees. The weather vane was so pivoted as to be free to point in all directions, up and down and sidewise. As might be anticipated, the vane when exposed in an open field began immediately to point in all directions, thus indicating that the course of the wind varies in both a horizontal and a vertical plane. The instrument is shown in fig. 1.

Not having an adequate intuition of the suddenness and frequency of the alterations in the direction of the wind, I had actually provided the anemoscope with graduated circles by which to read the direction from instant to instant, and had prepared a pendulum connected with an electric bell, which should beat seconds to thus enable an observer to record by hand the exact position of the vane for each second. But when the instrument was adjusted and exposed to a moderate breeze, it became apparent that no two observers could read and record the indications with the facility necessary to ensure accurate results for any extended period. I decided, therefore, to make it self-recording by combining it with a chronograph.

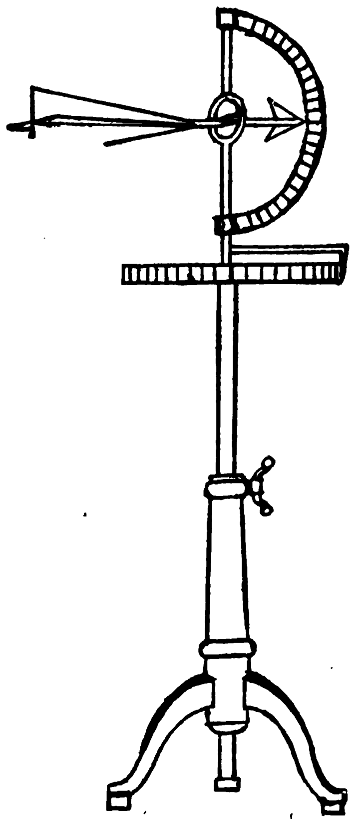


Fig. 1.

The anemoscope, as it appeared when mounted above the chronograph, is shown in fig. 2. The double vane has been replaced by two single vanes: one playing about a horizontal axis and mounted on the top of a vertical pipe, and the other placed 2 ft. beneath it and fastened rigidly to the pipe. The upper vane, which veers with the vertical variations of the breeze, communicates its movements to a fine steel wire running down through the pipe to one of the chronograph pencils; the other vane, which yields to the horizontal veering of the wind, turns the pipe with it, and the pipe moves a second chronograph pencil, as indicated in the figure. The third pencil shown in the illustration was intended to record the velocity of the wind by means of a fine wire connected with a pressure plate; but need not be considered for the present. The pencils in the background of

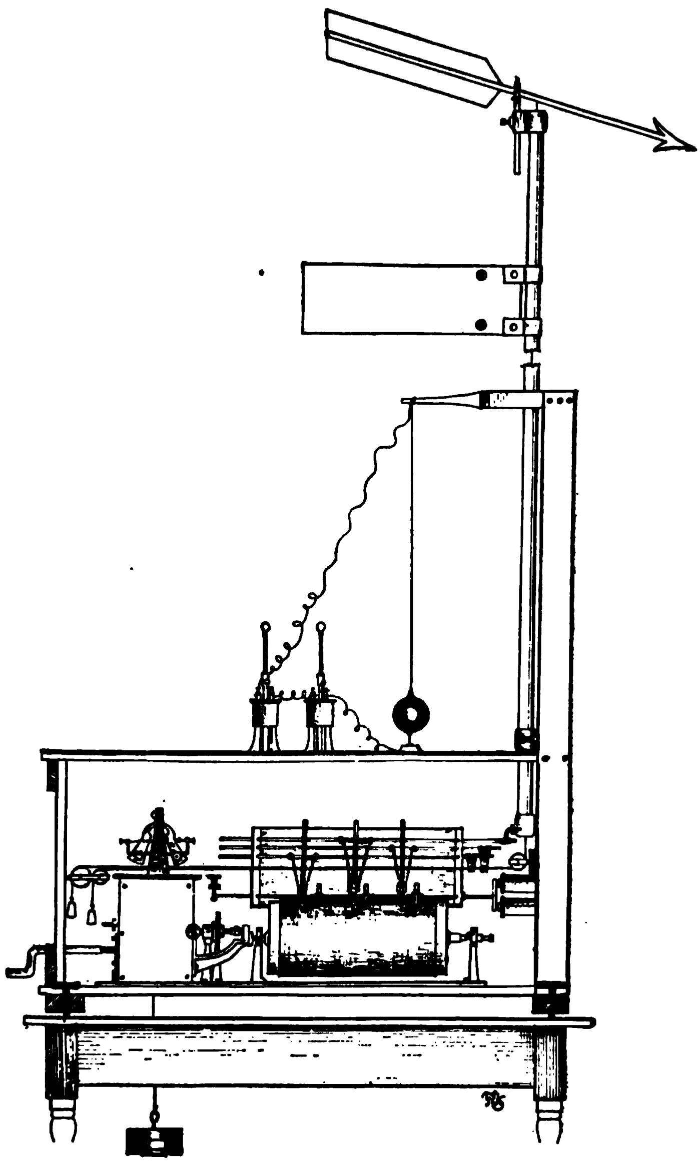


Fig. 2.

the illustration were employed to mark the lines of reference, and could, of course, be dispensed with by using sectioned paper. Pencils were employed, because the ink in the stylographic pens froze when exposed to the cold.

The instrument thus equipped becomes a universal recording anemometer, which, when delicately adjusted, reveals the complete history of the wind in its locality for any desired period of time; but as the attachment for measuring velocity was an imperfect one, I shall present only two of the records obtained. When I can again resume the work I hope to secure three records of indefinite length and in a breeze of any character. I have completed a design which will enable me to compact the entire apparatus within the space of a small clock and mount it at the top of a pole.

In order to secure sensitiveness in the instrument, anti-friction devices have been employed wherever practicable. The vertical pipe is supported on a ball bearing and set perfectly plumb by means of the pendulum and leveling screws; the upper rudder is supported on hardened steel pivots and adjusted to a perfect balance before use; the fine connecting wires pass over anti-friction pulleys. Thus the element of resistance to perfect freedom of movement becomes quite inconsiderable, and for the stronger breezes especially so.

The chronograph employed was one of a Swiss pattern, the drum measuring 6 in. in diameter by 14 in. in length, and rotating once a minute. The record thus obtained measured 1½ ft. for each minute, so that the minutest movement of the pencils could be easily distinguished. The paper used was 9 in. wide, and was fed from a roll supported on conical bearings at the back part of the box. When in operation the paper passed under the pencils at a speed of about 90 ft. per hour, and was afterward folded into book form for convenience of inspection. A chronograph of slower speed would have been preferable, since the record might well have been four or five times shortened.

As the drum could not be expected to rotate with perfectly uniform speed, a pendulum was used to mark seconds by swinging over a cup of mercury forming part of an electric circuit. Thus at each beat of the pendulum an electro-magnet, shown in the box enclosing the chronograph, jerked the sliding bar to which the reference pencils were attached, and produced a kink each second in the lines traced by them.

As before stated, the pencils were employed through necessity. The holders were intended to receive stylographic pens, but the observation being made in an open field in the depth of winter, no fluid ink was at hand which would resist the cold. Accordingly soft lead-pencils were screwed into the holders and weights placed on them to ensure distinctness of tracing.

The pencil holder is shown more minutely in fig. 3. It had to be so constructed that it could be readily moved to any part of the sliding bar to which it is attached, and yet remain free to rotate about the bar so that it could be quickly lifted from

the drum at any desired instant. It must also be capable of a slight elongation, so that all the pencils could, at the start, be placed on the same longitudinal element of the cylinder. The elongation is obtained by screwing the pencil in or out, thus lengthening the distance from the pencil-point to the sliding bar.

When finally the instrument was completed, about the middle of January, it was mounted on a table and placed in a sleigh, the chronograph being protected from snow-flakes by a plate of glass placed before the box containing it. Driving into the open country to avoid trees and dwellings, the sleigh was

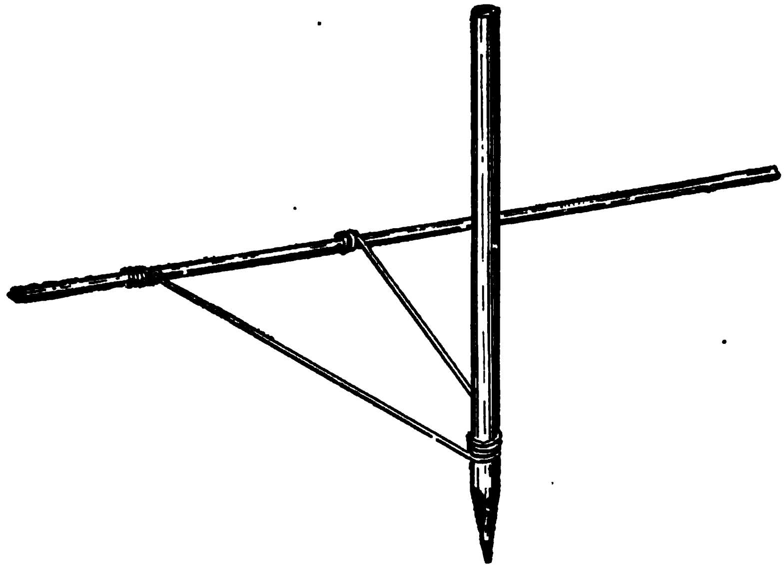


Fig. 3.

halted in a level locality between two clear meadows of 100 acres each. A breeze of 8 to 12 miles an hour was blowing from the northwest, the thermometer indicating 24° F., the barometer, 29.509 in. The pencils were found to work with perfect freedom and to feel the minutest impulse of the wind, as may be seen from a glance at the record, fig. 4. Even the momentum of the vanes, which I had feared might cause fluctuations in the tracings, was not apparent, either in the record or in the motion of the vanes themselves, save occasionally for a very sudden and extreme change of direction of the breeze. Fortunately no snow was falling to disturb the balance of the upper vane.

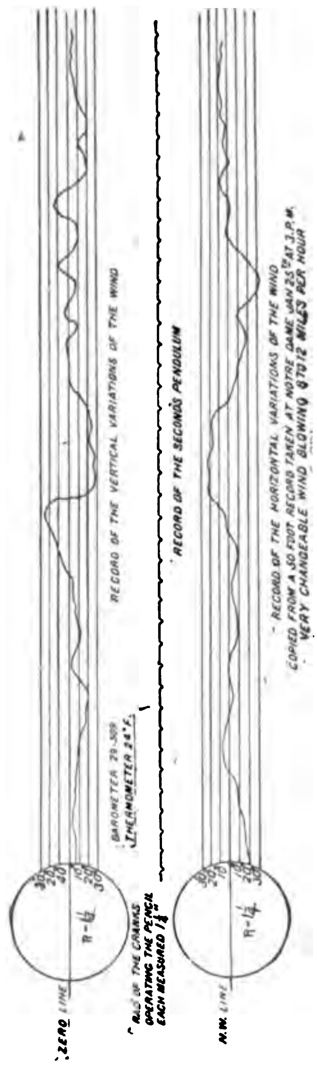

RECORD OF THE VERTICAL VARIATIONS OF THE WIND
BAROMETER
THERMOMETER 24° F.
RECORD OF THE SECONDS PENDULUM
RECORD OF THE HORIZONTAL VARIATIONS OF THE WIND
COPIED FROM A 50 FOOT RECORD TAKEN AT NOTRE DAME JAN 25TH AT 3 P.M.
VERY CHANGEABLE WIND BLOWING 8 TO 12 MILES PER HOUR
RAD OF THE CRANKS OPERATING THE PENCIL EACH MEASURED 1¼"
ZERO LINE
N.W. LINE
R-1¼
R-1¼

Fig. 4.

If we may form an estimate of the course of the wind from the record here presented, it would appear that a particle of free air blowing over an open plain near the earth's surface must follow a very wavy and irregular path indeed. It will be observed that both tracings vary by many degrees (frequently 20° to 30°) on either side of the reference lines, and that their alterations are not decidedly synchronous. I do not mean to assert that they are always entirely independent of each other, for it would require a long series of records to decide such a question; nor can I conjecture whether there should be a regular periodicity in either of the tracings. I have noticed quite frequently that a rise or lull in the velocity of the wind was accompanied by a corresponding variation in direction; but whether this be generally true must be decided by further observation.

One fact seems to be indicated quite positively—namely, that the wind veers every few seconds, and, as a rule, through many degrees at a time. This is perfectly manifested by any smoke stream blowing across an open country, and by the varying path of floating thistle-down. Thus one of the assumptions made to account for the phenomenon of soaring flight seems to have some justification.

The above record reveals nothing as to the other assumption—namely, that the wind, as a rule, varies greatly in velocity. But I have studied the velocity of the wind with a variety of anemometers, and am convinced that the assumption is a legitimate one for currents near the earth's surface.

My reason for not recording the velocity of the wind along with the direction-records above given, was that I could not contrive, nor find in the market, an instrument that could be relied upon to indicate the true horizontal component of the wind's velocity in a current of varying direction. All the anemometers of the meteorological observatories and of the instrument-makers have been calibrated in horizontal currents, and hence may not give the true horizontal component of a wavy current.

Fig. 5 shows the first pattern of anemometer made for use with the recording anemoscope. It is an ordinary, four-bladed mill intended to be fastened to the middle of the vertical pipe of the anemoscope to record the horizontal velocity of the wind. It may be observed from the figure that the anemometer shaft is provided with a commutator against which presses a brush from an insulated wire leading down to the chronograph magnet. The vertical pipe serves as the other wire. Thus each rotation of the mill is recorded on the drum. When exposed to the breeze this anemometer was found to turn with perfect freedom, and to record its rotation; but, like the anemometers of the Weather Bureau and all others of my acquaintance, it possessed too much inertia to yield perfectly to the sudden impulses of a fickle breeze; I therefore discarded it and sought another.

Apparently the most trustworthy instrument for gusty winds is one consisting of a light aluminum screw placed in-

side a cylindrical covering. A very neat anemometer of this type is constructed by the Richard Brothers, of Paris. Such an instrument, supported with its axis always in the direction of the horizontal component of the wind, was employed while the record above shown was in progress.

I have since learned from Professor Langley, of the Smithsonian Institution, that he has constructed a very light cup anemometer with which he has recorded the velocity of the wind from second to second. His results seem to completely justify the assumption which I have made in my theory of soaring flight regarding the suddenness and extent of the changes in the wind's velocity.

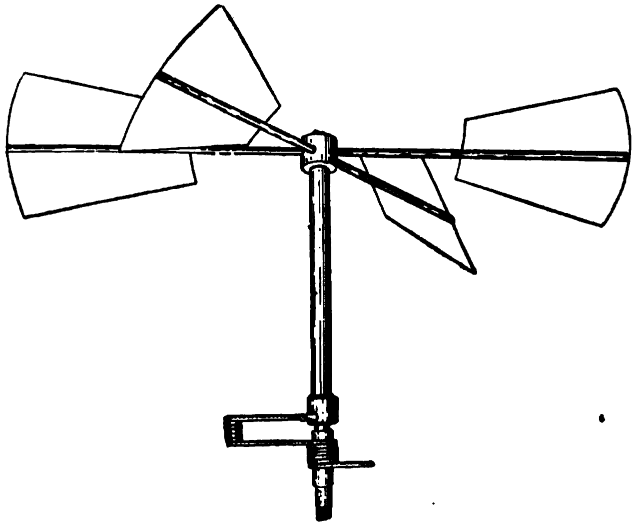


Fig. 5.

I have, likewise, received from Mr. S. P. Ferguson, of the Blue Hill Observatory, a copy of the Annals of the Astronomical Observatory of Harvard College, in which he recounts his recent measurements of the vertical component of the wind's velocity at Readville, Mass. The instruments employed by him were of the screw pattern, made by Richard Brothers, and quite recently placed on the market as "anemograph" and "anemo-cinemograph." As reported in the Annals, his records indicate that the wind played upward and downward with frequent alterations, but that its horizontal component was much greater than its vertical component. This is a result that we should expect at an altitude or in a very open locality.

While at the Johns Hopkins University last spring, I employed an exploring line to indicate to the eye the waves of a changeable air current. It consists of a strong fine thread, having attached to its extremity a small rubber balloon inflated just sufficiently to fairly float. When paid out in a breeze the balloon rises and falls with every billow like a cork on the water, and the line itself is bent into waves, sometimes of monstrous size, thus enabling one to form a conception of the actual billows of the atmosphere.

I do not mean to assert that the direction of the line accurately portrays the direction of the wind at all its parts, for the pull of the balloon tends to straighten the line; but I am sure that it does not give an exaggerated indication, because the pressure of the wind must always be against the concave parts of the line; hence the wind's course must be even more wavy than the line itself. If the main exploring line had along its length a number of short branch lines, each tipped with a small balloon, the branches would point out the direction of flow in their immediate locality.

After some preliminary tests from the top of the Physical Laboratory of the Johns Hopkins University, during the Easter vacation of 1893, I ascended the Washington Monument at Baltimore, where I paid out the exploring line at a height of 200 ft. The wind was blowing toward the southeast at the speed of 25 to 35 miles per hour, and the sky, which had remained clear till 3 o'clock, was rapidly darkening, with indications of approaching rain. The balloon when let forth immediately fell to a depth of 30 or 40 ft., being caught in the eddy of the monument, then presently encountered the unbiased current, sailed in it toward the southeast, approximately level with the spool end of the thread. After the balloon had drawn out 100 ft. of thread I checked it to observe the behavior of this much of the exploring line. The balloon rose and fell with the tossing of the wind, but did not flutter like a flag, as it would do if formed of irregular outline. Neither did the thread flutter, nor do I believe there is ever a tendency in a line to greatly flutter in a current as does a flag or sail. Presently I paid out 300 ft. of the exploring line, whereupon the waves in the thread became quite remarkable. The thread then, as a rule, was never approximately straight. Sometimes it was blown into the form of a helix of enormous pitch; at other times into the form of a wavy figure lying nearly in a single vertical plane; and again the entire exploring line would veer through an angle of 40° to 60°, either vertically or horizontally. The balloon, of course, seldom remained quiet for more than a few seconds at a time, but tossed about on the great billows like a ship in a storm. Quite usually the billows could be seen running along the line from the spool to the balloon, and, as a rule, several different billows occupied the string at one time.

The observations just delineated, however curious they may be, afford no adequate conception of the behavior of the air currents over an open plain, nor at a great height above the

earth, because the Washington Monument of Baltimore stands but 100 ft. above the surrounding buildings, which undoubtedly send disturbances to a greater height than 200 ft. To supplement these explorations, therefore, I determined to have them repeated from the top of the Washington Monument at Washington and the Eiffel Tower at Paris.

Toward the latter part of the Easter vacation I again let forth the exploring line from the top of the Washington Monument at Washington, at a height of 500 ft. A stiff breeze was blowing from the northwest, and, as the locality is quite free from obstructions, everything seemed favorable for an exploration of the free air. But, unfortunately, I had not taken into consideration the enormous magnitude of the monument and of the consequent eddies surrounding it. Accordingly, when the balloon was let forth from one of the windows, it became involved in a large and violent eddy from which it could in no manner be extricated. Rising vertically upward from the window to a height of some 25 ft. it encountered the direct current and sailed toward the southeast with great rapidity to a distance of 30 or 40 yds., then suddenly turned to the right, being caught in a mighty whirlpool which sucked it downward through an enormous spiral path to a depth of 100 ft., and again threw it upward to the top of the monument, thus returning it quite near to my hand. After witnessing these evolutions for 10 minutes I was obliged, by the lateness of the hour, to return to the elevator without having observed the behavior of the exploring line in a direct current. I saw, however, what precautions would be advisable to ensure the success of a second attempt.

If it be granted that the wind blows in gusts of varying velocity and direction over a considerable part of the earth's surface, it must be evident that a consideration of their action is of the utmost importance in the study of aerial navigation, and especially of the questions of equilibrium and propulsion. I dare say the success of open-air experiments often depends as much upon the behavior of the air currents as upon the proportions and adjustments of the apparatus employed. Doubtless many of the strange phenomena which have hitherto puzzled experimenters with sailing and flying apparatus, the sudden pitchings, the "aspirations," and the mysterious soarings, can be attributed directly to the fickleness of the wind.

SAILING FLIGHT.

From Observations made at Constantine, Algeria.

By J. Bretonnière, C.E.

INTRODUCTION.

Ever since the year 1889–90, when I published in *L'Aéronaute* (an illustrated periodical of the French Aerial Naviga-

tion Society) an article on Sailing Flight, I have continued to direct my attention to the manœuvres of "sailing birds." This was an easy task for me, since Constantine, the city in which I live, is (it seems to me, at least) a locality specially favorable for the observation of sailing flight. In fact, it is by hundreds that one sees here, wheeling overhead, from March to September, the *storks*, whose nests are built on the housetops, and the *Egyptian vultures* (néophrons Percnoptères), whose eyries are in the cliffs of the great ravine which partly surrounds our city. In their tournaments, too, though in smaller numbers, the *bearded griffins*, which nest in the mountain of Sidi M'cid, come each day to take their part. The great *tawny vulture* displays himself frequently, and at times in numerous bands. If to these birds, which are really "soarers," and which never have recourse to beating their wings, except when positively forced to do so, one adds (since they also animate the sky of Constantine by their flight) certain species which can soar, but yet frequently beat their wings, such as the *carrion-crows*, which nest, but in small numbers, in Sidi M'cid; the *ravens*, whose babbling flocks shelter themselves under the bridge of El Kantara; the *falcons*, whose eyries are in the ravine, and whose scattered bands practise everywhere their abrupt flight; and also innumerable *martinets* and three kinds of small *swallows*, whose rapid beat of wings does not prevent their excellent sailing, it is evident that the observer has here an abundance of subjects for study.

The *storks*, the *Egyptian vultures*, and the *bearded griffin* have been particularly the objects of my observation. The *stork* especially, as on account of the simplicity of its flight, he had given me my first tuition.

One object I had in view was to determine whether a protracted and more attentive study of facts would confirm the theory which had been suggested to me by the laws of the action of aerial currents upon surfaces, as expounded by writers on aviation, and which had given me the first insight into the performances of "sailing flight."

These performances, as I have said, could be observed everywhere about me, both near and far, but I soon saw that observed at a distance they were not disclosed with the distinctness one should wish for; and that when near by, they were often disturbed by an element which it was very difficult to eliminate in a region so hilly as that of Constantine, where the currents of air were almost always different in direction from those prevailing up aloft.

For the purpose of analyzing soaring flight, in the horizontal currents which constitute the great majority of winds, and which are contemplated in my theory, I was led to observe the perturbations caused by local currents produced by obstructions and their influence upon sailing flight near the ground. The following describes some of the observed facts and conclusions.

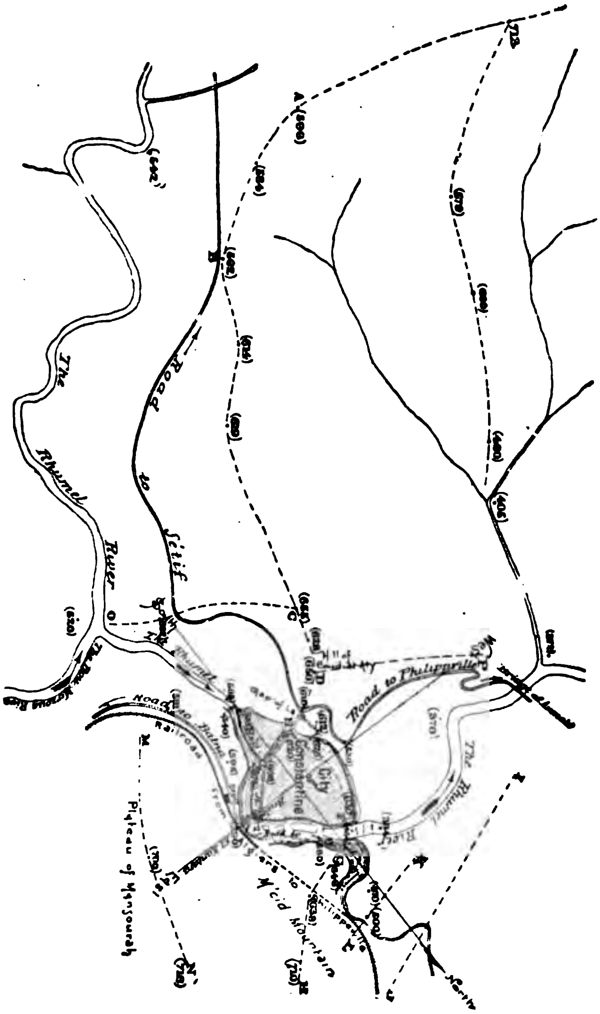
In order to render sufficiently clear what I have said about the currents resulting from certain inequalities of the earth's

surface, it has seemed to me necessary to place before the eyes of the reader a plan of Constantine and its environments (page 175), indicating the relief of the ground by writing the altitude of the principal points. Such a plan ought, also, to afford us the means of describing more precisely the manœuvres I have witnessed. Moreover, since the winds and the performances of sailing flight which depend upon them obey invariable laws, this plan ought, I think, to allow my readers, who may so wish, to verify, in part at least, the facts which I am about to describe.

The region of which Constantine occupies the center presents a deep depression, whose trend is directed nearly from the southeast to the northwest, and is represented to the southeast of Constantine by the river *Bou-Merzoug*, and to the northwest by the river *Rhumel*. This depression is cut transversely by a line of high points, whose crest, marked *A B C D E F G H*, passes through the city of Constantine itself. This line forms a barrier against the prevailing winds of the locality, those blowing from the north and west and those from the opposite direction. This barrier is intersected at only one place by a gorge, 150 m. wide and 200 m. deep, terminating at the point bearing the mark 454, and affording a passage for the united waters of the *Rhumel* and *Bou-Merzoug*. The defile of "La Brèche," situated to the southwest of the city, nearly in the axis of the great depression mentioned above, and whose altitude is 610 m., is 100 m. higher than the bottom of the valley up stream from the city, and 240 m. higher than the same valley below the city. The difference, 140 m., represents the sums of the rapids and falls of the river, the last of which, situated above the point marked 454, has an altitude of about 70 m.

I ought to mention, as having an effect on the wind in the neighborhood of Constantine, that besides the barrier *A B C D E F G H*, there are also the counter-forts *C O* and *D P* which form spurs, and the important counter-fort *M N* (the "Mansourah"), and also the two secondary counter-forts, *I J* and *K L*, the three latter, like the line *A B C D E F G H*, extending back to meet the great chain of mountains which, at the northeast, command Constantine.

The city of Constantine is situated upon a steep limestone crag, whose inclination is in a contrary direction to that of the valley, and which is, so to speak, detached from the neighboring mountain by the gorge which I have mentioned above. The abrupt ravine thus formed circles the city for more than half its perimeter. Its depth varies, in consequence of the contrary slopes of the river and of the plateau which serves as the seat of Constantine, from a depth of 50 m. above the city to a depth of 200 m. below it. It forms in the midst of its course a right angle, and at this point it is spanned by a bridge 120 m. in length (the bridge of "El Kantara"), which connects the city with the railroad station. Before the construction of this work Constantine was accessible only toward the ravine of "La Brèche," the abrupt edge of the calcareous plateau bordering the precipice everywhere else.



MAP OF THE CITY OF CONSTANTINE, ALGERIA.

Before relating the actions and performances of sailing flight which I have witnessed, I will reproduce, but in a more concrete and concise form, the theory which I published in *L'Aéronaute*, and I will describe the observations of the winds which I have made.

THEORY OF GLIDING FLIGHT.

Gliding flight, according to the meaning which I believe to have been attached to this term originally, and which I adopt, comprises both the manœuvres of the bird in the wind, which seamen have called "sailing flight," and the simple gliding in calm air; all the movements, in a word, which the bird executes by assuming the form of an aeroplane—that is to say, by holding his wings simply outstretched. We call those birds which practise this kind of flight, "sailing birds."

The air is an elastic fluid exerting, upon the plane surfaces of bodies which strike it, a pressure whose intensity varies with the extent of the surfaces and as the square of the velocity of impact. This effect increases rapidly, and can give, with a rapid velocity, a substantial support to the bird's wing.

An important point to remember is, that the velocity to be considered in the action of the air upon such surfaces is not singly either that of the air or that of the body, but their relative velocity. For example, if a bird has a velocity of its own, v, in a current of air of velocity v', and in an opposite direction, then the velocity to be considered will be the relative one $v + v'$.

A flat body subjected to the pressure of the air obliquely, either by the force of its own weight or of its kinetic energy, or of an extraneous impulse, experiences a tendency to glide and does glide, if no other force opposes the first one. I shall cite an example. Suppose one takes a draftsman's rule, flat and flexible, and uses it as a saber to strike a sudden blow toward a definite point. The rule, if its plane be in the direction of the object aimed at, will go directly toward it. It will undergo, on the contrary, an energetic deviation if its plane makes a decided angle with the direction in question.

Various experimenters have established formulæ which enable us to calculate the normal or oblique action of the air against plane surfaces.

Sailing flight has long been considered as mysterious and inexplicable. Who knows, it was said, whether the bird may not be sustained in the air by means of some properties of its feathers still unknown, by movements which are imperceptible to our eyes, because the bird practises them too far away from us? Nevertheless, at Constantine at least, the sailing bird passes at times very close to the observer, often within 20 yds., and one does not notice any particular movement of its feathers or of its wings, at all adequate to explain its flight.

In my article, published in *L'Aéronaute*, I began by showing that it is not necessary to seek so far for an explanation which is close at hand, and that the bird gliding in calm air does not behave otherwise than as a simple aeroplane. I had

often directed my attention to the stork, and, after a certain number of observations, I have concluded that the velocity of this bird, during its flight in a straight line and in calm air, must be about 20 m. per second, and that the inclination of its path, comprised between 15 per cent. and 20 per cent., might, without grave error, be estimated at $17\frac{1}{2}$ per cent., an inclination corresponding to an angle of about 10° below the horizon. Having had a stork in my possession for a time, I weighed and measured it. Its weight was found to be $3\frac{7}{10}$ kilograms (8.14 lbs.)—its wing-spread $1\frac{6}{10}$ m. (5.25 ft.) and its horizontal surface, with outspread wings, seven-tenths of a square meter (7.53 sq. ft.).

I tried then to design an aeroplane, not exactly of the identical form of the bird (a form which it would not be possible to determine precisely and to submit to calculation), but possessing the same conditions of weight, of length, of surface, of thickness, and form of cross-section, in so far as I could realize these things. The aeroplane sought for presented itself to me in the form of a simple poplar board 1.6 m. (5.25 ft.) in length, 0.44 m. (1.44 ft.) in breadth, 0.014 m. (0.55 in.) in thickness, presenting a sharp edge whose angle was 40°. I chose poplar wood, because with its density of .388 and with the dimensions adopted I reproduced the weight of the stork. I ascribed to my aeroplane, as a matter of course, the faculty which the bird possesses of maintaining its equilibrium in the air, and of selecting and retaining whatever angle it chose above or below the horizon.

It is manifest that such an aeroplane, abandoned to itself in air, and maintaining a certain angle below the horizon, ought to descend by virtue of its weight, repelling the cushion of air which it compresses, and at the same time gliding on this cushion of air. This effect, which appears evident *a priori*, has, moreover, been established by numerous experiments. When the air, driven backward, exerts an upward pressure against the aeroplane equal to its own weight, and at the same time the resistance which its cross-section encounters equals the force which urges it forward, then the movement becomes uniform and the path becomes a straight line. It is during this period of uniform movement that we have to consider the aeroplane if we wish to compare it with the stork, inasmuch as we only observed the stork during its uniform movement and not at its departure. Selecting, now, one of the formulæ given by experimenters, to express the action of aerial currents against inclined planes, and applying it to the particular case we are considering, we arrived at the conclusion that our aeroplane, repelling the air at an angle of 5°, must follow a path inclined about 10° below the horizon, with a velocity of 20 m. (65.6 ft.) per second. These results appeared to me, and they will, I think, appear to the reader, to show as a consequence that the bird behaved simply as an aeroplane in the movement which we have considered, and without doubt also during the average of its sailing flight—that is to say, as a body acting upon the air through its sur-

face, its velocity and its weight, and possessing with its velocity a kinetic energy which can be converted into work. In truth the principle just announced may be admitted as a postulate, and what has preceded is less a demonstration than a verification of observed facts by the aid of the formulæ.

We give the name of "kinetic energy" to the power which a body possesses in consequence of its motion. It is given by the formula $\frac{1}{2} m v^2$, in which m represents the mass of the body and v its velocity. The mass of the body is the ratio, $\frac{p}{g}$, of the weight of the body p to the acceleration of gravity g, whose value is approximately 9.81 in metric units and 32.2 ft. in British units. The kinetic energy is that which the body would possess if it fell *in vacuo* from a height of $h = \frac{v^2}{2g}$. It represents also the work which a body possessing such velocity is capable of performing. It has theoretically the power to raise the body to the altitude $h = \frac{v^2}{2g}$; that is to say, to the altitude from which it should have to fall *in vacuo* to acquire the kinetic energy $\frac{1}{2} m v^2$.

Must we admit, as a consequence of the mechanical conceptions which I have just recalled, that the bird may rise to the same height as that from which it would have to fall to acquire the velocity which it possesses? No; this effect cannot be entirely realized for two reasons: the first is, that the altitude h, corresponding to the kinetic energy of the bird $\frac{1}{2} m v^2$, is less than the real height of fall, a part of the theoretical fall having been lost through various causes, particularly because of the resistance of the air to the hull of the bird; the second is, that in order to transform into altitude, by whatever means, the kinetic energy $\frac{1}{2} m v^2$, still another part of the altitude, $h = \frac{v^2}{2g}$, must be lost on account of the inefficiency of the output as compared to that demanded by theory.

The means by which I conceive that the bird may transform its "kinetic energy" into altitude is to tilt above the horizon the angle at which its aeroplane was inclined below the horizon during its gliding in calm air. Just as it glided, while descending, by virtue of its weight over the cushion of air which it compressed beneath it, just so under the action of its kinetic energy, when its angle of flight becomes inclined above the horizon, it will glide and elevate itself on the cushion of air which it will compress before itself.

In this manœuvre we must not expect, as I have just said, to realize a complete transformation of the kinetic energy. But what will be the efficiency of this transformation? I confess that I do not know, as there are no experiments, to my knowledge, which may enlighten us on this point. Still we

have some useful information regarding this; it consists in the description which the old falconers have left us of a performance which they called the *passade*, a manœuvre employed by the falcon to seize its prey when the latter is flying beneath it. In the *passade* the falcon, without beat of wing, precipitates itself from a great height and recovers altitude by an abrupt movement called *ressource* (from the Latin *resurgere*), and it then may repeat the movement several times; for its first attempts do not always succeed in seizing the prey. In the semi-circumference which it thus describes in its fall and its recovery, the falcon seems to find in the elasticity of the air a recoil which enables it to regain the altitude whence it has descended. This description leads us to believe there is no great loss of effect in the manœuvres by which the bird, gliding at a great velocity, glances off upon the elasticity of the air, whether these manœuvres be performed in a vertical plane, as in the *passade*, or whether they be executed in a horizontal plane, where there is simply a change of direction. But there are no data which will enable us to indicate, even approximately, a coefficient of recoil for the work expended by the bird upon the air.

We have considered the bird as gliding rapidly downward on the air under the action of its weight. This gliding is a primal element of sailing flight. It alone is sufficient to enable the bird to accomplish a great number of his manœuvres. It produces the velocity which results in the action against the air, the only point of support which the bird can have. Thanks to this velocity the stork, for example, in its ordinary gliding course, descends only about 3.5 m (11.5 ft.) per second, or a fraction of the amount which it would descend if it fell vertically with outstretched wings. The bird can thus, without great loss of altitude, sustain itself in space for a time and profit by the gusts and ascending currents which it encounters. And, finally, this gliding will serve while tracing the zigzags and orbits whose effects I shall presently describe.

Another primal element of sailing flight is the gust of wind which is also termed a "squall." It gives the bird the power of elevating itself. It is evident that the bird which descends on its inclined course in calm air can at any point whatever of its path suddenly assume an angle above the horizon and elevate itself for an instant, by reason of the velocity which it possesses; but this velocity will soon be spent, and it will then be obliged to have recourse again to downward gliding. It can at most, if it resorts to a series of such manœuvres, preserve as a mean result of its flight the same average descent—that is to say, as slightly inclined as its ordinary course. But let us suppose that from time to time a gust of wind occurs having a velocity v'. At the arrival of the "squall," the relative velocity of the wind with respect to the bird, which was v, will become $v + v'$, and its kinetic energy, which was $\frac{1}{2} m v^2$, will become $\frac{1}{2} m (v + v')^2 = \frac{1}{2} m (v^2 + 2 v v' + v'^2)$. If the bird, upon the occurrence of the wind gust, inclines his aeroplane above the horizon, he will ascend above his trajectory to

a height corresponding to the increase in its kinetic energy, or $\frac{1}{2} m (2 v v' + v'^2)$. This increase may be considerable, as will appear, for example, if we assume $v' = v$. The kinetic energy of the bird will increase in this case from $\frac{1}{2} m v^2$ to $\frac{1}{2} m \times 4 v^2$—that is to say, it will be quadrupled, and its net increase $\frac{1}{2} m \times 3 v^2$ will be equal to three times the kinetic energy of the bird before the "squall." It appears, then, that if gusts of wind of considerable force occur frequently the bird can elevate itself rapidly into the air. I do not think that the squalls given by nature, as in the case we have supposed, play a considerable part in sailing flight. Nevertheless, what I have just said of gusts of wind possesses great importance in my theory, as we shall soon see.

The bird which follows a course represented in plan by a straight line cannot expect, therefore, to gain altitude, unless it encounter a squall supplied by nature or an ascending current of air.

If the bird is sailing in an ascending current, the mass of air which surrounds it will exert its influence. The downward inclination of its course will be either diminished or transformed into an upward direction. The stork, for example, will gain in altitude if the vertical component of the ascending current is superior to 3.5 m. (11.5 ft.) per second, the amount which it descends in its ordinary gliding. In certain cases of ascending currents a very singular effect may occur. This is when the bird has a course directly opposed to that of the wind, with a velocity equal to that of the current of air; then at the same time that the bird tends to descend in the mass of moving air, the air tends to carry it along in the contrary direction, and the bird, to the eye of the observer, and in reality with reference to a fixed point on the earth, will not change its place. Unless through the effect due to the vertical component of the wind, the bird which follows, in an ascending current, a course whose projection in plan is a straight line, cannot gain further altitude. It can, as in calm air, acquire momentarily a slight elevation by assuming an angle above the horizon, but it must afterward lose it in a more decided downward gliding, which becomes necessary to recover its velocity.

In a horizontal current, and for the reason given above, the bird which follows a path whose projection in plan is a straight line cannot increase its altitude; but such a current, by transporting the bird along with it, will give to its path certain peculiarities which may be indicated. If the bird glides in the same direction as the current, its path will have less inclination. For example, the path of a stork gliding with a velocity of 20 m. per second, in a current of the same velocity, will have an absolute speed of 40 m. per second as regards the earth, and its rate of fall will be 3.5 ÷ 40 m. instead of 3.5 ÷ 20 m., its inclination in calm air. When the bird glides against the wind, the inclination of its path will be increased if the wind has a velocity less than its own. If the velocity of the current of air be greater than his own speed, the bird

will be carried backward. Finally, if the velocity of the wind and that of the bird flying against it are equal, the latter will, to the eye of the spectator, placed at the same level with it, and in reality, be under the same conditions as if it fell vertically on outstretched wings, and its velocity of fall will not be different from that which it experiences when gliding in ordinary calm air. To the observer placed below the bird it would appear not to change place at all.

The advantages which a path, represented in horizontal projection by a straight line, can procure in sailing flight are, then, limited indeed.

We shall now see how they may be increased by an artifice of the bird in resorting to zigzag and spiral flight, manœuvres the latter of which occupies a special place among the enigmas which this Sphinx of the air presents to man.

Flight, and that which pertains to it, is still to a great extent an unsolved problem. From the beginning of time the bird has practically solved questions of which man to-day barely begins to understand the theory. Still we ought not to be astonished that such is the case. The bird frequently performs its evolutions very far from our eyes, and if it does not try to conceal its secrets, it is too wary of our actions to lend itself closely to our observation. The problems, moreover, which concern its flight have in the past never much interested the great majority of men; boldness in research not being then, as it is in our own day, justified by numerous and incessant discoveries. Finally, the light fluid which serves as a support to the bird is not very ostensible to our senses, and even the elements of the laws which govern it have been understood but very recently. Only two centuries and a half separate us from the epoch when the suction of the pump was explained by the horror of nature for a vacuum. Quite recently men found some difficulty in accounting for the movement of that singular projectile, the boomerang, which, after having struck its game, returns to the feet of the hunter. The weapon seemed marvelous, but our astonishment arose chiefly from the fact that it was solely in use among the natives of Australia, the lowest of the savage tribes. How, it was asked, could men barely deserving the name of man make such a discovery? Why, I ask in turn, should the inventor of the boomerang need to understand the theory of his instrument any more than the inventor of the pump? The theory of the boomerang seems sufficiently clear now; and from the explanations which are given, this weapon seems to be nothing but a sort of aeroplane.

Man has, then, without suspecting the theories which appear so interesting to us now, frequently discovered devices which experience alone could reveal to him. In the case which we call the solution of the problem of sailing flight the bird has done likewise. It has, in the adventures of flight, found beneath its wing the means of ascension, and it continued to have recourse to the same means when it wished to gain a greater altitude.

The artifice of the bird consists in transforming into relative squalls—that is, into means of ascension—the horizontal aerial currents, in the midst of which it would otherwise have to follow a path of rectilinear horizontal projection and be incapable of elevating itself.

Let the bird, at the expense of its altitude, perform a glide, $A B$, fig. 1, across a wind of velocity v', and then, with the velocity v acquired in this first movement, suppose it to turn abruptly, following the line $B C$; facing the wind with an angle of flight above the horizon, it will have created for itself the relative squall of which we have spoken.

Let us examine the consequence of this manœuvre, and in order to study the question more easily, let us assume, in the first place, that the mechanical transformation of the bird's energy is complete. All losses of effect through friction, resistance of the air to the passage of the hull of the bird, change of direction, etc., being thus eliminated, we have nothing left to consider but the theoretical relation of effect to cause. The altitude h, expended to obtain a velocity, v, will then be equal to $\frac{v^2}{2g}$; and a kinetic energy $\frac{1}{2} m v^2$, employed to obtain an ascension, would then procure an altitude $h = v^2 \div 2g$. But the velocity of the bird leaving B in the direction of C will be, without reduction, $v + v'$; and as, on the other hand, we have seen that the pressure of the air, and, consequently, the mechanical effect to be obtained, will be the same for the case in which the relative velocity is composed of the two velocities v and v' in contrary directions, as for the case in which the bird, with a velocity of its own equal to $v + v'$, encounters calm air, then the kinetic energy of the bird projecting itself from B toward C will become $\frac{1}{2} m (v + v')^2$, and the altitude which it can attain will be: $H = (v + v')^2 \div 2g$. The bird whose velocity, v, has cost him an expenditure of altitude in going from A to B equal to $v^2 \div 2g$, and which will gain from B to C an altitude $H = (v + v')^2 \div 2g$, will thus gain by the relative squall so created an additional height, $H - h = (v + v')^2 \div 2g - v^2 \div 2g = (2 v v' + v'^2) \div 2g$, a result already arrived at for the natural squall.

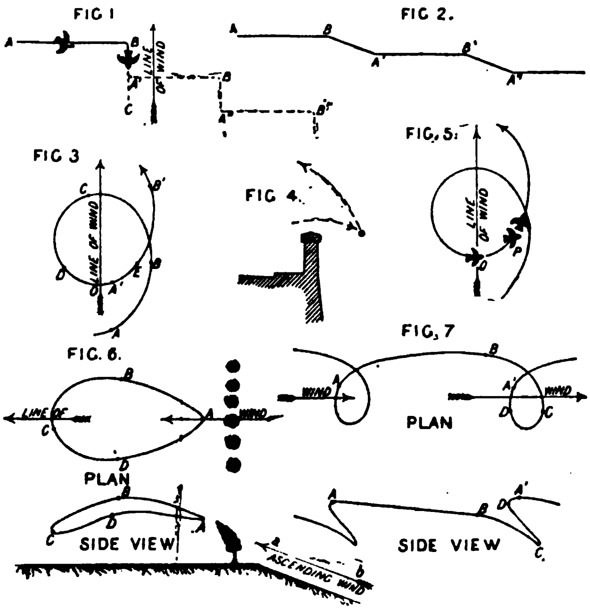
In what has just been said, we have supposed that the bird, leaving the point A with no velocity, is to use up, during its ascension, all its kinetic energy. But we may conceive that it already has, at the point A, an initial velocity V_1 corresponding to an altitude h_1, and that the bird follows a broken line, $A B A' B' A'' B''$, creating a series of relative squalls, and thus retaining at its disposal, for the continuation of its movement, a velocity at the points A, A' and A'' equal to V_1. Let us, as in the preceding example, designate by v the velocity of the bird, as it arrives at the point B, and by H the total altitude which it is destined to attain above the point B. In the present case H will be equal to the altitude which the kinetic energy of the bird would enable it to attain $= (v + v')^2 \div 2g$, diminished by the altitude $h_1 = V_1^2 \div {}^2g$, corresponding to the

velocity V_1, which it must reserve at the point A' in order to continue its course to B'. The fall from A to B will be equal to $h - h_1 = v^2 \div 2g - V_1^2 \div 2g$. The gain in altitude by the relative squall will then be, under these circumstances,

$$\left[\frac{(v + v')^2}{2g} - \frac{V_1^2}{2g}\right] - \left[\frac{v^2}{2g} - \frac{V_1^2}{2g}\right] = \frac{2\,v\,v' + v'^2}{2g},$$

just as in the case of the hypothesis previously examined.

It is to be noted that this gain of altitude, given by what I have termed a relative squall, increases, not only with the



velocity, v', of the wind, but also with that of v, which the bird acquires in gliding downward. The bird may then, if it finds it to its advantage, acquire high speed and realize in a single manœuvre a considerable gain of altitude by plunging down at a steep incline; a thing which it can easily do by diminishing the extent of its outspread wing surface.

It may be interesting to the reader to put in numbers the results given by the formula $H - h = \dfrac{2\,v\,v' + v'^2}{2g}$. Let us suppose the current of air, v', to have a velocity of 10 m. per second (22.8 miles per hour), which is quite an ordinary wind,

and let us begin by assigning to the bird, a velocity, v, of 20 m. It can then, by means of a "relative squall" effected under these conditions, obtain an increase of altitude of $\frac{2 \times 20 \times 10 + 10^2}{19.62} = 25.48$ m. (83.57 ft.). It would rise by the quantity $\frac{2 \times 30 \times 10 + 10^2}{19.62} = 35.68$ m. (117 ft.), with a velocity of 30 m. per second, if there were no losses and no head resistance.

We have now studied the effect of the "relative squall," supposing the mechanical transformation to be complete. We ought, in order to obtain a complete elucidation of the subject, indicate also the coefficient of reduction to be applied to the formulæ, but unfortunately, as I have said, this coefficient is not yet known.

The "relative squall" is practised by the bird under two forms, the zigzag and the orbit. I apply the term "zigzag" to the manœuvre in the form of a broken line, which has just been described. It may consist in more or less angles, and be traced with the angles more or less open. The figure, No. 1, represents the zigzag under its most emphatic form, the angle $A\,B\,A'$ then being a right angle; but the bird may not need all the altitude which this form would communicate to it. It may, for example, have as the object of its journey to follow a horizontal line, the direction of which coincides with the mean of its general course.

In this case, instead of turning (after gliding) in a direction directly opposite to that of the wind, it may limit itself to crossing the current of air obliquely, as indicated in fig. 2. It will then encounter only a fraction of the velocity of the wind, and it will receive but a part of the gain in altitude which it might be able to realize; but this part will be sufficient for the end in view. This last manœuvre may, I think, aid the birds in accomplishing their long peregrinations. The zigzag may thus at times consist in a series of very slight angles, which only attentive observers will notice.

The orbit is the spiral path of the birds, which we see them describe in the heavens as a sort of helix, drifting in the same direction as the wind.

The bird glides downward across the wind from B to C in the orbit (see fig. 3); from C to D it performs a rapid ascension, by attacking the current of air with an angle of incidence above the horizon. From D to A' it may rise still further a little, and begin to descend when it leaves this last point; then to resume at B' its downward transverse gliding.

The form of the orbit, however, is not always the same; for it depends upon the wind, upon the intention, and upon certain qualities of the bird.

The orbit which has just been described is that which the bird ordinarily traces in order to elevate itself with a horizontal wind of moderate velocity.

Sometimes on leaving D the bird, having perhaps already spent its kinetic energy, glides downward to O, and thence rises again to E, whence it begins to descend again. The orbit has in this case two summits, D and E, and two depressions, C and O.

The orbit traced in calm air can consist only in a downward gliding, permitting the bird to descend gently from a great height toward some point situated below it—toward its nest, for instance. If the orbit be traced in an ascending current, not vertical, the upward component of the wind raises all parts of the helicoidal course, even those otherwise covered in downward gliding, and, if this component is sufficient, the latter portions cease to be inclined downward; they are simply less ascending than the others. As to the horizontal component, it confers on the orbit a part of the required ascensive energy. In a vertically ascending current the orbit consists in a simple gliding, as in calm air. It will be ascending if the velocity of the current is sufficient; for the stork, for example, if it is more than 8.5 m. per second. In a whirlwind capable of raising it into the air, the bird need only to select and to preserve a convenient place.

The orbit, as I think, serves mainly to enable the birds to gain that great altitude in space where they are seen to disappear, in order, no doubt, to reach by gliding, with or without zigzags, the course which will conduct them to the object aimed for.

It has been said, *apropos* of zigzags, that the bird may wish to follow a route through space, at always about the same altitude, and that in this case it would adap the form of its "relative squalls" to this purpose. I think likewise that the orbit may become for the bird simply a means of horizontal translation in the direction of the wind. The bird in that case need not rise, in its return against the wind, more than the altitude lost in downward gliding. The return in each orbit will then be a very slight recurvity in horizontal projection, a mere loop, and the path will be composed chiefly of downward glides at a flat angle of descent.

In the theory which I have just developed, the part which I consider as new, and peculiar to myself, is that concerning the "relative squall."

I give below a *résumé* of this manœuvre, which seems to me to be the fundamental basis of sailing flight.

The action of the impact of the air against a flat body increases as the square of the velocity, hence this fluid can, if the velocity be great, give a substantial support under the wing of a bird. The velocity to be considered in this action is neither that of the air nor that of the body exclusively, but the relative velocity of the one with reference to the other. This relative velocity will be the sum of the velocities of the body and of the wind, if these two velocities be in opposite directions. The bird is nothing but a flat and heavy body which can attain a great velocity by gliding on the air. When it possesses velocity it can also, by gliding against the air with

an angle above the horizon, transform its kinetic energy into ascension.

As a consequence of what precedes, the bird, which, after its gliding in calm air, whence it has acquired a velocity v, encounters a gust of head wind of velocity v', is in the same condition with reference to its ascensive force as if with a velocity $v + v'$, and consequently a kinetic energy $\frac{1}{2} m (v + v')^2$, it were to breast a calm. If, on encountering the head wind mentioned, the bird should assume an angle above the horizon, it could realize, barring the reduction for loss of effect, the transformation of its kinetic energy into ascension, and as a consequence into a gain of altitude.

There is reason to believe, from the effects of the *passade* spoken of by the old falconers, that in the bird's change of direction there cannot be a great loss of velocity, and that a bird which has a velocity, v, across a wind of velocity, v', should, on turning against the wind, have a velocity nearly equal to $v + v'$.

The sailing bird understands how, by an artifice, it may create in a horizontal wind a "relative squall" which will yield it the same advantage as the natural squall. This artifice consists in a glide downward *across* the wind, followed by a return *against* the current of air with an angle above the horizon. It obtains in its gliding the velocity v, and expends in altitude $h = v^2 \div 2g$. In its attack on the wind it spends the kinetic energy $\frac{1}{2} m (v + v')^2$, which gives it an altitude $H = (v + v')^2 \div 2g$, and it thus obtains, by means of the "relative squall," a gain of altitude $H - h$, which, neglecting the reduction for loss of effect, will be equal to $\dfrac{2vv' + v'^2}{2g}$.

OBSERVATIONS OF THE WINDS.

More than once when I began to understand the theory of sailing flight I witnessed facts which I could not explain and which left me in doubt. Theory told me that the bird ought to glide rapidly in order to sustain itself in the air, and yet I saw, in the great ravine which borders Constantine, below me from the point marked (608) (Place Pérégaux), or above me from the point marked (580) (the Cornice Road), some Egyptian vultures and some falcons, which had a very slight horizontal velocity, and almost at times none at all, and yet were sailing without descending, and often even ascending. Theory tells us, moreover, that in the "orbit" traced in a horizontal wind the path of the bird is at one time inclined downward and at another time upward, and yet right before my eyes an Egyptian vulture rose from its eyry (placed midway up the ravine a short distance below the Place Pérégaux) without any part of its path having been inclined downward, and I saw also above the point marked (470) some bearded griffins describe orbits ascending in all their parts. Was my theory, then, erroneous, or did there exist in the places where I saw these performances, which seemed to me to be paradoxical, peculiar

effects of sustentation, due, for example, to ascending currents? By and by I began to perceive that such currents existed under certain circumstances, and I could then account for the manœuvres which at first seemed inexplicable.

The air, on account of its transparency, does not permit us to perceive its disturbances even when most violent. But vapors, clouds of smoke, clouds of dust, and light objects, such as leaves, straw, down, floating seed, flakes of snow, and so forth, which it sometimes carries with it, can, if one pays attention to them, depict the movements of its great squalls and of its lesser undulations.

The majority of my readers have no doubt noticed that the eddies of wind which are found near buildings, in the street, and at cross-roads, in the neighborhood of cliffs, and even near slight elevations of ground, have not generally the same direction or the same intensity as the wind which prevails in the upper atmosphere. But what laws have given rise to these secondary currents? It may at times be quite difficult to explain the formation of an eddy, such as manifests itself in the presence of any special object. In fact, not only by the side of the building directly struck by the wind are there different currents produced, varying according to the points of observation, but the different currents, due to the impact against this building and those produced in the neighborhood by other buildings, or elevations of the ground—nay, by reacting against one another, give rise to the most unforeseen effects. In very hilly localities one is often surprised to encounter progressively, currents entirely opposed to one another. If one traverses the "Cornice" road one finds, 20 m. on this side of the point marked (580), a strong wind from the rear, and at 20 m. on the other side of the same point a head wind of equal force. In the space of a half circle, which this road describes, around the point marked about (470), one encounters during the same walk, by a north wind, all the blasts of the wind chart.

But if the small obstacles that exist on the surface of the earth, the houses and rocks, give rise to blasts of winds which are often confused and difficult to explain, the greater undulations of the ground, the mountains, the deep ravines, the basins of rivers, give rise to powerful currents which can be perfectly explained and which are clearly defined. When the wind encounters the mountains in its path, the mountain deviates the current if it strikes obliquely. If it strikes transversely, then while the upper strata of air continue their course without resistance, the lower strata, to which the mountains offer an obstruction, are elevated in vigorous ascending currents parallel to the ground, and thus sweep over the crest of the mountain. Beyond this the currents spread about in various directions, some in descending blasts, which follow the inclination of the ground, but are rather feeble. If the wind ascends a ravine or the basin of a river, it follows the sinuosities in its change of direction, thus transforming itself near the earth into ascending currents whose directions are very different from those in the higher regions of space. If it descends the ravine or

the river basin, it follows it with its various windings, but with descending and feeble currents. Along the flanks of abrupt cliffs struck by the wind, vertical currents are formed having at times an extreme violence. The winds which ascend ravines which are deep and well enclosed give rise to ascending and even to vertical currents, not only along the walls, but oftentimes even in the middle of the ravines.

A great number of my readers have, no doubt, observed with their own eyes the movement of the air currents which I have just indicated. As for myself, over and above the action exerted on the birds, it is chiefly clouds of dust and smoke that have enabled me to determine them. The explosion of gunpowder blasts on the side of Sidi M'cid, which excavated the "Cornice" road, have not been without instruction to me. Quite frequently during the winter I have been able to judge of the effect produced by the west wind upon the high cliffs which border the works of the road. On certain days the shovelfuls of earth which the workmen threw over into the ravine were driven back by the ascending wind with extreme force, and twice it became necessary to suspend operations on account of the sand blowing upward in the face of the workmen and blinding them.

The very day that these lines were written, in a snow-storm and a strong northwest wind, a friend of mine, who lives in a house situated at the summit of the slope which descends toward the Aumale Bridge, said, in presence of some persons living in the center of the city, "Here the snow falls from the sky upon your heads; at my house it rises from the Aumale Bridge."

Although it has been easy for me to perceive the existence of layers of ascending air upon the sides of mountains, I have not been able to estimate satisfactorily the thickness of these layers. I think that the thickness ought to increase with the altitude of the mountains obstructing the wind. I think also that certain ascending strata of air do not limit their ascension to the level of the mountain top, but that, urged up by the pressure from below, they continue to rise above the crest.

But what altitude may they reach? To what distance in front or behind may their disturbance extend? I am not able to say. The fact that the upward action does not cease at the level of the crest is proved to me, because vertical currents formed along the sides of cliffs by no means stop at the top of the cliff. I have sometimes seen clouds of dust raise vertically fully 20 or 30 m. above the crest. It is, moreover, a matter of experiment which every one can make for himself where the wind strikes a high wall surmounted by a parapet. If one throws a ball of paper over the parapet (fig. 4), the wad will be caught by the wind and carried up, so as to fly back over the parapet in a curve, often passing several meters above the head of the observer.

One may inquire also as to the height attained by the action of the current of air, which often rises from the middle of the ravine in which the wind is enclosed. I have often seen light

objects rise from the great ravine of Constantine to a distance of 30 m. above the top of its sides, but I think that the action must extend considerably beyond this altitude. My own observations in support of this latter opinion are limited to the effects produced upon the flight of birds. These effects, which I regard as the action of ascending currents, I have seen clearly defined over the great ravine mentioned above, at an altitude of 50 m., and to the right of the "Place Pérégaux;" also at an altitude of 100 m. and to the right of the point marked (580 m).

On this subject I found some interesting information in the description of a balloon voyage. The aeronauts remarked that the wind carried them over a very long course, following a path, at a height of 500 m. from the earth, constantly above the windings of a river whose basin was bordered by mountains. The directing influence of a river basin may, therefore, under certain circumstances, govern the currents of air up to a height of at least 500 m. above the bottom of the valley.

If, as we have just seen, the greater inequalities of the earth's surface can give rise to ascending currents differing greatly in direction from the winds prevailing aloft in the same region, and differing likewise from each other, we may conceive that two of these currents may encounter one another—for example, at a gap where each should arrive in ascending opposite ravines. What will be the result of their union? Will it be an ascending wind, having as its direction the resultant of their two directions, or will it be a rotating wind? I think that one or the other will be formed, and most probably the vortex. Among the localities about Constantine, where I can see that such an effect may be produced, are, first, the abrupt circular space which surrounds the point marked (470); and, second, the region which extends to the southeast and in the neighborhood of the crest comprised between the points marked (584) and (619).

As to the first, if the wind arrives from the northwest, the currents of air, having followed the bottom of the valley, will be transformed into ascending blasts, having at the time of their passage above the point marked (470), a direction parallel to $K L$. But when these blasts have attained an altitude of 100 m. above the point (470), they will be met obliquely by the northwest wind blowing over the crest $K L$. The result will, I think, probably be a whirling wind.

In the second locality the same kind of effects, but more important and more frequent, are to be anticipated from the north wind; because the masses of air in motion in two different directions are more considerable and may have more points of contact. On the one side the basin of the Rhumel will guide southwestward, the aerial currents coming from the junction of the Rhumel and the Bou-Merzoug. On the other side the north wind will arrive without horizontal deviation, but in ascending blasts, from the points 372 and 405, toward the crests which extend between 584 and 619. In passing over these the rising stream will meet obliquely at an angle of about 45° (horizontally) the currents of the Rhumel. The neighbor-

hood of the depression marked 562 appears to be specially designed by nature for giving rise to eddying winds.

Similar phenomena are often produced within range of our observation without our eyes having the slightest perception of them. But if the whirls find in their path some objects which they can carry along with them, then their existence is made manifest.

I saw one day a long and narrow object, resembling a rumpled newspaper, rise from the point 470, of which I have just spoken, and above which some bearded griffins had produced those orbits ascending in all their parts which I have mentioned above. This object rose by fits and starts rapidly on a direct vertical line, in which it remained suspended in the direction of its length. When it arrived at a height of about 300 m., it was caught by a horizontal current and carried above the point 560, where it fell swiftly. I think the presumed newspaper occupied during its ascension the axis of a whirlwind.

On another day, from the "Place Aumale," in the neighborhood of 620, in Constantine, I observed one of those little balloons, filled with hydrogen, which are given as toys to children. Having escaped no doubt from the hands of its little master, it came from the northwest, following a horizontal direction, and passing a dozen meters above the houses. Having arrived to within a short distance of the "Place Aumale" it stopped, and I saw it diminishing rapidly in size and disappear in the zenith. I thought that the little balloon must have been seized and transported upward by a vertical current. Above Constantine, but I could not say that it was precisely above the "Place Aumale," I have several times seen birds sweep in orbits not only ascending in all their parts, but even with a vertical axis, which, to my mind, indicates that the bird is immersed in a whirlwind also vertical.

At the point marked 700, ascending currents or whirlwinds resulting from the blasts coming from the two slopes of the plateau of Mansourah can, within my knowledge, be produced by a southwest wind. My attention was, moreover, drawn to this place from the beginning of my investigations, on account of the attraction which it appeared to exercise at times on certain sailing birds. During an excursion which I recently made in the region where the point marked 600 is situated, I saw, rising from the point 700, what appeared to me to resemble an immense column of smoke a score of meters in diameter and about 200 m. high. I thought that the forage stacks of the cavalry quarters, which are near the point 700, were on fire. But presently the column disappeared, just as whirlwinds of dust disappear; and in reality it was nothing but a dust whirlwind which had attracted my attention for five or six minutes, and which had perhaps existed for some minutes before I perceived it.

If the reader, from what precedes, is inclined to conclude with me that the inequalities of surface which come under my observation can, under certain circumstances, give rise to as-

cending blasts, he will admit also, I think, that in those regions where, from the distance, we cannot perceive clouds of smoke and dust; and where moreover these witnesses are doubtless lacking, there should also be produced, under the influence of the great masses of the mountains, ascending currents proportional in mass and duration of action to the accidents of the ground.

There is also another well-known cause which may produce some ascensive effects, and that is the heat of the sun, which, by warming strongly the layers of air which are at the surface of the earth, render them less dense and give them a tendency to rise. I mention this fact without attaching to it any undue importance in accounting for sailing flight.

There may be other causes capable of producing ascending winds and whirlwinds, for we know as yet very little concerning the movements of the atmosphere.

What I have said concerning the wind may be summed up in a few words. The greater inequalities of the ground transform the lower layers of the prevailing winds into ascending currents, and these sometimes, by their mutual reactions, or by the action upon them of the generating winds above them, give rise to new currents more markedly ascending, or to whirling winds or whirlwinds.

There likewise occurs descending currents, but these are relatively feeble, and the bird, who gets no good from them, will generally avoid them. Still, at times it may be forced to undergo them.

One sees what an important part the ascending currents may play in sailing flight, by bringing beneath the bird's wing a vertical component, and how the whirlwind can raise it up rapidly in the air. Also how, in the study of this kind of flight, an ignorance of such facts may create perplexities and cause errors.

THE EVOLUTIONS OF SOARING FLIGHT.

I have, in my theory, supposed the sailing birds as performing long glidings in calm air, or in a horizontal current, or in ascending currents, or either utilizing a squall given by nature, or having recourse to the artifice of a "relative squall" either in "zigzags" or "orbits."

Now if we consider the actual flight of the bird we shall observe, succeeding one another in various ways, straight lines, broken lines, and the convolutions of the "orbit." But does the bird in its different evolutions demonstrate the laws which I have developed in my theory?

For the most part it is very difficult to determine this. The observer lacks too many of the elements which are necessary in order for him to solve this question. He does not always know, with a sufficient certainty, the direction of the current of air at the precise point where the bird happens to be. Still less does he know its intensity. His eye cannot judge (an important point) the inclination of the plane of the bird's wing above the horizon. Oftentimes he cannot conjecture the ob-

ject which the bird is aiming at, and yet it would be useful for him to know it, for the bird which descends toward the earth for the purpose of finding prey or of returning to its nest does not behave in the same manner as one which sets out for a long excursion, or as one which, making a long voyage, has no other object, during the few moments which mark its passage to the observer, than to traverse space. In many cases the observer cannot tell whether the bird is rising in its path or is descending. He will doubtless perceive, clearly enough, whether the bird has gained or lost in altitude after a certain number of manœuvres; but, during the greater part of the path traversed, the observer will not be able to affirm whether the bird rises or descends, and this it may be necessary for him to know in order to be sure whether the manœuvres practised by the bird conform with my theory. As the bird is generally above the horizon, the observer will rarely have before his eye points of reference permitting him to appreciate the gradient of the path which he is considering. He is, moreover, most frequently hampered by an error of perspective, due to the fact that the bird is not on the same level with him. The bird, when it is at a higher level than that of the observer, may appear to rise when, in point of fact, it approaches the latter, and to descend when it really recedes from him. On the other hand, this effect of perspective is reversed when the bird—a thing which rarely happens—is below the observer.

In spite of these difficulties and causes of error, a prolonged and attentive observation and fortunate circumstances have enabled me partly to verify my theory. I shall present the evolutions of sailing flight, which I have been able to observe, in the same order in which my theory considers them.

1. The long gliding: in time of calm, in a horizontal wind, or in an ascending current.
2. The "squall" supplied by nature.
3. The "zigzags."
4. The "orbits."

The long glidings in calm air are exhibited principally when the bird descends in the morning, from the lofty bluff on which Constantine is built, toward the plains that extend to the northwest or the southeast of the city, or when, at night, it returns to its nest from the side of the mountains which border the horizon toward the northeast. It is chiefly the stork which I have observed performing such glidings.

In a horizontal wind I have seen the sailing bird, having sufficient altitude, descend toward its nest, gliding in a long straight line, but with varied directions with respect to the wind. At times of violent head wind the bird, if I were below it, seemed immovable, and, if I were about on a level with it, it seemed to descend slightly. These last observations, however, never lasted more than a few seconds; the bird, which doubtless found no profit in maintaining this position, performed a "zigzag," an "orbit," or a wing beat in order to terminate it.

In currents ascending parallel with the ground the bird frequently produced long straight lines, in which it utilized the vertical component of the wind. These facts I have observed on the slopes which incline from Constantine toward the point marked (870), on the declivity which extends from the point *D* toward the north, and also on the flanks of the buttress *C O*, likewise exposed to the north. One day when, with a north wind blowing, I was posted for observation between the points 592 and 655, I saw the storks, whose nests are in the lower part of the city, and the Egyptian vultures, who have their abodes in that part of the great ravine near the point 505, execute the following manœuvre. As they wished to set out on an excursion, and the north wind produced a descending current over the inclined plateau supporting the city, thus hindering them from taking their soaring start above the place, they hastened to reach the declivity north of the buttress *C O*, ascending along this side, by a simple gliding in the ascending wind, as far as the point 655, where, in a horizontal wind, they then commenced to trace their orbits.

In an ascending wind I have frequently remarked the hovering action, which I have spoken of in my theory, the bird holding itself head on to the wind, and if not absolutely immovable, at least not rising or falling more than a few meters at most, according to the slight variations of the wind. One day, from the point 608 (Place Pérégaux), I remarked two Egyptian vultures performing this manœuvre several times in succession, and during a period of 30 seconds each time. These birds were compassing this kind of balancing, above the point 595, in a northwest wind, which must have produced an ascending wind, according to the inclination of the ground, of 10° to 12°. I have even seen domestic pigeons, a species which is not classed among sailing birds, remain suspended above the point 612. They returned several times to assume before me, for five or six seconds, this same position, which one might call immovable, since the variations hardly exceeded 1 m. in horizontal and vertical projection. It is true that these pigeons held themselves at the top of the very marked ascent which extends from the point 470 to point 612, and in addition were somewhat back of and above a belt of eucalyptus-trees, whose tops, bending beneath the wind, doubtless furnished to the bird's wing a very strong vertical component.

I need not mention the descending currents, except to say that sailing birds experience only injurious effects from them. I have seen storks, or vultures, which, finding themselves caught in a descending head wind, were obliged to flap constantly because the benefit of "zigzags" and "orbits" would have been neutralized. A bird accidentally encountering such a wind is deflected in a remarkable manner in its passage; but the birds know somehow how to recognize the places in which such winds are to be found, and almost always avoid them.

The case of the gust of wind which I set forth in my theory, in order to arrive at a conception of a "relative squall," may

I think, be sometimes observed in nature. I have not, however, any case of this kind to relate, as having been specially remarked by myself. But I have observed other natural squalls utilized by the bird—those, for example, in which the bird, having performed a glide sheltered from the prevailing wind, arrives at a point where this wind has full scope. In the great ravine of Constantine I have several times seen a sailing bird, after a downward glide toward the northeast along the right-hand cliff, find itself suddenly, at the moment of its arrival above the point 454, caught by a strong west wind. Turning itself at once head to the wind, it made a rapid ascension of from 20 to 30 m. in height. At other times and in the same parts of the same ravine, but after having been first sheltered along the right-hand cliff, it rose by making an attack against the north wind.

I have said that the bird which descends can and does follow, in any direction whatsoever, a straight line across a horizontal wind. I have never seen a bird which wished to ascend have recourse to straight soaring. It always follows broken lines or orbits. The broken lines, then, it seems, serves the end the bird has in view—that is, to gain altitude.

Constantine, the abode of the majority of the birds which I have observed, is for them chiefly a place of departure and arrival. I have never seen migrating birds, on their extended excursions, traverse its sky. Three or four times I have noticed birds very high in space, which passed on beyond, but did not seem to heed our locality, in which they were doubtless strangers. Did they perform "zigzags"? I cannot affirm it positively, but there were changes in the glint and aspect of the planes of their wings, probably corresponding to changes of direction.

As to the birds which I have been able to observe close at hand they really sail on broken lines, but did they trace these zigzags according to my theory—that is to say, by first gliding downward *across* the wind and then turning *against* it with an angle of incidence above the horizon?

I must admit that I have not been able to establish whether the birds, which trace slight zigzags, glide along first on a right line across the wind and then attack it obliquely, with a gain of altitude in a second right line. As there was no point of reference in the sky, it was difficult for me to appreciate any slight descents and ascensions. I only made sure that the directions of their path as a whole, generally oblique to the wind, would permit them to adopt this manœuvre.

But it is quite otherwise when, instead of these zigzags at obtuse angles, we consider those very marked tackings whose horizontal projection is a right angle, or nearly so. The bird has recourse to these very often, and executes them in such a way as to leave the observer in no doubt as to the manner of performing them, which is exactly that of my theory. In special cases the return against the wind is so decisive that there results ascension at an angle of about 45° above the horizon.

In order to enjoy the spectacle of these "zigzags," one needs but to go, in time of a rather strong north wind, at the hour when the storks and the Egyptian vultures return to their nests, and place one's self to leeward of the wind with reference to the city—for example, between the points 628 and 655. The birds, which by their downward gliding in a head wind have lost considerable altitude, are obliged to recover it. The "zigzags," the simple "glidings," and at times an "orbit" mingle with and succeed one another, and finally the bird, having shown that it knows how to overcome the wind, arrives at its abode. Generally in its gliding across the wind the bird seems to advance sideling, with the tip of one wing in advance. In reality I think it presents its head to the direction of the "relative" wind.

Certain birds, among others the bearded griffin and the Egyptian vulture, perform, as if in play, a manœuvre which in its theory belongs to the "relative squall," and which, if it be not in reality the *passade* mentioned by the old falconers, presents at least a great resemblance to it. They precipitate themselves down from a height of 30 to 40 m., in order to raise themselves again to a greater height by opposing to the wind their ventral surface.

The path of the bird which follows an "orbit" is a kind of helix, deformed and drifted by the wind, and, as it were, depressed by this drifting.

I have said in my theory how I conceived, from the beginning, the different forms of the "orbit," as well in its horizontal projection as in the distribution of its inclines upward and downward, its summits and its bottom points.

The facts which I have observed since then have only confirmed my first conceptions.

The bird seems to trace its orbit by turning indifferently, either in the same direction as the hands of a watch, or in an opposite direction. After a series of "orbits" in one direction, it may then turn in the opposite direction. It does not always, in order to commence its helicoidal movement, wait for a sufficient velocity of wind. By a stroke of the wing, from time to time, it supplies the insufficiency in the velocity of the current of air. The diameter of the orbit in horizontal projection is quite variable. I may assign to it as a mean, I believe, about 60 m. if in the direction of the wind, and about 40 m. if across it. There are some orbits having smaller diameters than these, and others again having much larger widths.

All the birds which I have observed have, I think, recourse to the ordinary orbit with one summit. I have never noticed any but the Egyptian vulture sweep an orbit with two summits, and only the stork makes use of the long orbit with a short loop.

Moreover, none but the stork, and that only occasionally, have I seen appear to fluctuate in its attitude, which is ordinarily inclined transversely toward the central part of the helicoidal path.

The bird having arrived at the point *O*, fig. 5, where its axis

is perpendicular to the direction of the wind, preserves its axis parallel to this position all the way to the point P, where, suddenly, it again becomes tangent to the direction of movement.

In 1891 I saw some domestic pigeons sweep an orbit which, I think, ought to be described here as an evolution of soaring flight, notwithstanding it was not performed by sailing birds. A band of about 25 pigeons, wishing to remain in the neighborhood of a point where it evidently found something to feed upon, swept and re-swept an orbit always the same, and such as I present in fig. 6 in plan and elevation. At the beginning and end of each orbit they remained poised, as already described, suspended head to the wind at A, just above the point (612), during a period of from five to six seconds. Then, turning abruptly, they traversed under the waning influence of an ascending wind $a\ b$, the line $A\ B$, itself slightly ascending. They then glided downward from B to C, but from C to D effected a marked ascension, and, on leaving D, flapped their wings to regain the point A.

In order to observe them, I was posted at the point (625)—that is to say, just at their own level, and at an average distance of 80 m., and, therefore, in a good position to appreciate the form of the curve traversed by them.

In May, 1892, a stork traced above me, under conditions less favorable for observation than those of the preceding case, but still close at hand, one of the elongated orbits of which I have spoken. The loop of this orbit, which I show in plan and elevation in fig. 7, was described at about 80 m. from me. From A to B the bird glided down with a slight inclination. From B to C it descended more rapidly. From C to D it rose at an angle of 45° above the horizon. From D to A' a further slight ascension was effected, and beyond A' the slight downward gliding recommenced. The pronounced descent $B\ C$, and especially the rapid rise $C\ D$, were very clear to my perception.

The preceding observation was the most precise that the sailing bird had thus far afforded me, regarding the form of the orbit, and it appeared to me to be precious evidence, when, a month afterward, I conceived the idea of applying to the observation of "orbits" a process which would yield numerous and relatively precise results. This process permits the direct delineation on paper, and then the reproduction in relief before our eyes, of the approximate geometrical form of the bird's helicoidal path.

It is accomplished as follows: I hold in my left hand, in the direction of the bird which is tracing its orbits, a sheet of paper applied on a field book, taking care that the sheet is quite vertical in the direction of its length, and that the hand which supports it makes no movement. I support the back of my right hand against the sheet of paper. A very short pencil, but .02 m. (0.8 in.) in length slipped between the middle and forefinger, and held in place by the thumb, traces on this sheet of paper a conical projection of the bird's path. A light sliver of wood or a simple straw, sufficiently long to

project above the sheet of paper, say 0.10 to 0.15 of a meter (4 to 6 in.), is held by the pressure of the thumb against its lower end, which is in contact with the top of the pencil. This straw ought to be always held quite vertical, or at least travel quite parallel with itself, and its upper extremity, forming an indicator, is moved to follow the path of the bird, and thus constantly to be on the straight line which connects the eye of the observer remaining in a fixed position with the bird moving in the air.

While the indicator describes, on a small scale, the movement in space under the eye of the observer, consisting in the conical projection of the bird's path, the pencil traces on the paper a parallel and equal projection. For each operation executed in this manner I take care to note the direction of the wind, the direction of rotation of the bird's orbit, and the angle above the horizon at which the base line of the observed paths has appeared.

This first part of my proceeding having been performed, there remains to reproduce in relief the real form of the path.

I construct beforehand, of soft galvanized iron wire,* two helices about .03 of a meter (1¼ in.) in diameter, the one right-handed and the other left-handed. Taking the one in my hand whose direction of rotation corresponds with that of the bird, I hold it up in such a manner as to be, relatively to my eye, at about the same angle above the horizon as that observed for the base line of the path, and then, fashioning it to the suitable inclination and pitch to correspond with the projection taken in the field, I slightly crush the upper part of the spire, and I have reproduced in my small helix of iron wire a representation of the observed orbit.

The procedure just described is intended to co-ordinate the different parts of the path, and to dispose of the difficulty mentioned above, of want of reference points in the sky. It may, moreover, be modified, in its vertical projection, for example, in the following manner: The eye, instead of being stationary, might move, accompanied by a pencil pressing on a pane of glass, which would thus receive the outline of its passage. The indicator would be a fixed point. This might be the top of a stick, or any other point, placed at a convenient distance from the eye, in order that the figure traced by the pencil might have any desired dimension. We might thus have a larger drawing, and one free from some of the imperfections which may be produced by the method indicated above. It is true that the figure obtained would be the inversion of the one sought for, but to obtain this it would be sufficient simply to turn the glass over.

I have traced in pencil and reproduced by means of iron wire the form of a great number of orbicular paths, taken in different parts of the sky, as chance presented them to me.

A comparison of these trajectories presents the important

* Lead wire would probably be preferable.—Ed.

fact that they are included almost exclusively in two groups, about equal in numbers.

The trajectories of the first group are formed of "orbits" similar to those which I have described as being traced in a horizontal wind. They appertain to my theory, and they confirm it.

The trajectories of the second group exhibit the bird as rising in the sky in a course ascending continually, and at times so steeply that the pitch of the spire is equal to five times its diameter. I think that the bird is at such times manifestly under the action of a whirling wind.

In addition to these two groups there appear at times trajectories whose orbits indicate in the wind the presence of a vertical component, more or less marked.

The equality in numbers of trajectories in the two groups just mentioned might lead us to believe that whirling winds are very frequent in the atmosphere. I do not think this is true, even in the region of Constantine, where the inequalities of the ground, to which I chiefly attribute them, may produce them in greater numbers than in many other regions; but the sailing birds, which understand how to derive advantage from them, know also where to find them.

I have found, it is true, in different parts of the sky trajectories belonging to the second group, but the region extending above the valley of the Rhumel, between the points (612) and the junction of this river with the Bou-Merzoug, is the one which has furnished them most abundantly. The trajectory with quite vertical axis marked *F*, in fig. 9, was observed from the point (591) looking in the direction of the point (612), and was performed probably in its neighborhood.

My observations have shown me the orbit under six distinct forms. Six trajectories, whose figures were constantly found beneath my pencil, are reproduced in the two diagrams, figs. 8 and 9, given herewith, representing each one orbit of these forms, which are:

For fig. 8, *A*, the ordinary orbit with one summit, in a horizontal wind.
" " " *B*, the orbit with two summits (of the Egyptian vulture) in a horizontal wind.
" " " *C*, the very long orbit (of the stork) in a horizontal wind.
" " 9 *D*, the orbit in an ascending current.
" " " *E*, the orbit of the trajectory with inclined axis in a whirling wind.
" " " *F*, the orbit of the trajectory with vertical axis in a whirling wind.

Each one of the diagrams above corresponds to a reproduction in relief in a small box, which can be transformed into a frame, by unscrewing the bottom and opening the lid, which I send with the present paper. On the open lid of the box are reproduced the figures drawn in the diagram, and correspondingly, inside the box, are fixed the soft iron wire helices, by the aid of which I am enabled to represent in relief the

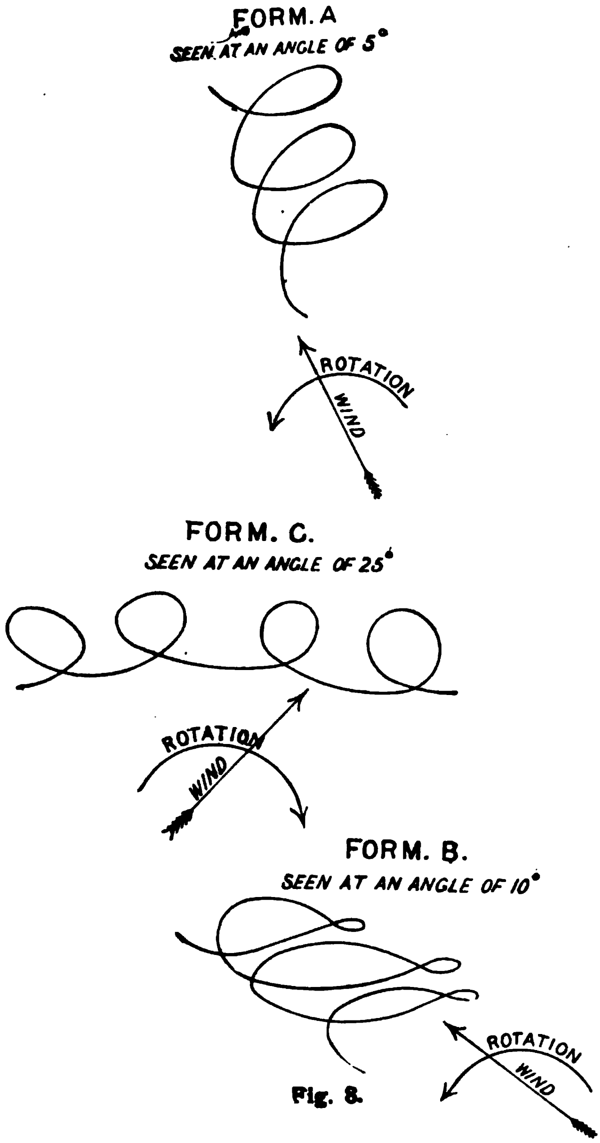

FORM. A
SEEN AT AN ANGLE OF 5°
ROTATION
WIND
FORM. C.
SEEN AT AN ANGLE OF 25°
ROTATION
WIND
FORM. B.
SEEN AT AN ANGLE OF 10°
ROTATION
WIND

Fig. 8.

trajectories corresponding to the figure.* The bending of the wires has not been without its difficulties. On the other hand, upon such a small scale I could not at all hope to obtain an exact reproduction of the details. As regards the main features, I think I have approximated to the truth.

The orbit of the trajectory D seems to demand special attention. Of the two branches which chiefly compose its outline, the horizontal one is but that part of the ordinary orbit glided over downward, which is elevated by the vertical component of the wind, so as to become nearly horizontal; the other is the ascending part of the same ordinary orbit heightened by the same component so as to become almost vertical.

If the spectator will place his eye on a level with the plane

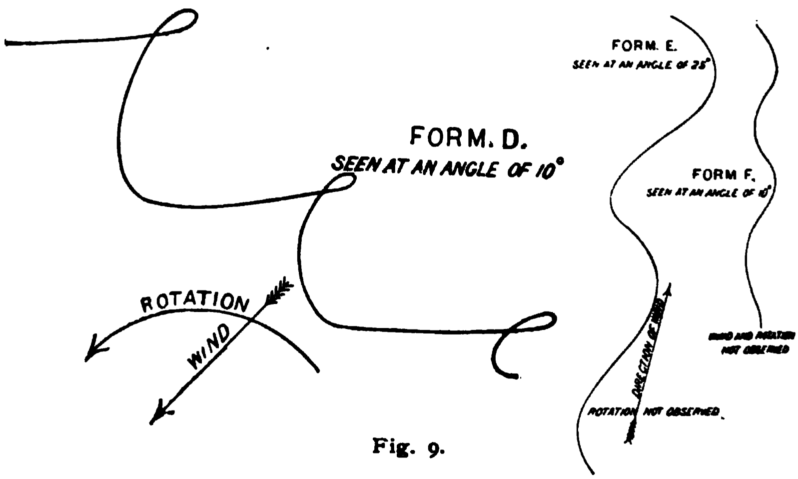


Fig. 9.

of the base of one of these boxes, and on the center line of this base about .25 m. (10 in.) from its front boundary, he will see the small helices present in perspective forms quite the same outline as those mounted on the lids.

In point of fact, the small helices will then be observed by him at the same angle at which their corresponding trajectories were observed by me in the sky, and his eye will be at the same distance from each of them that mine was (at the moment of observation) from the indicator, or the top of the straw following the bird in its flight.

I shall terminate the description of the facts which I have observed by mentioning a very curious spectacle which has at

* The two boxes above described were passed from hand to hand at the conference, and attracted great attention.

times attracted my attention, and which is connected with the action of ascending winds and whirling winds. All of one part of the sky is sometimes occupied by a legion of sailing birds or occasional soarers, the rest of space appearing deserted. Storks, vultures, crows, hawks, martinets, and small swallows, soaring or beating their wings, are carried along in an ascending movement. It was in such a gap into the heavens that I saw described, for the first time, an orbit ascending in all its parts. The true sailing birds, such as the stork, Egyptian vulture (Pharaoh's chicken), the bearded griffins, all have the habit of directing themselves toward the point where they see other birds describing their orbits, expecting (at least so I have interpreted their manner of acting) to there encounter conditions more favorable for soaring flight. In the particular case which I am describing, this kind of attraction was exerted upon all the birds which practise sailing flight to a greater or less extent. They hastened to profit by a pleasure train which a whirling or at least an ascending wind has placed at their disposal. Sometimes a tinge of dust has risen on such occasions, to confirm my conjecture regarding the phenomenon which invited the voyagers. For the most part, however, the air is as clear there as elsewhere. One day, from the eastern extremity of the bridge "El Kantara," I saw a winged multitude sporting in a cylindrical region over the flanks of the Mansourah plateau. It was the hour at which the storks return to their nests. Several of these birds had arrived, tracing, as usual, their long glides. All of a sudden I saw a stork come on a line which apparently crossed the animated cylinder. Was it going to pass by it? Quite otherwise. It entered in the midst of the whirling throng and partook also of the movement of its new companions, doubtless not wishing to lose an occasion to perform an ascension which would cause it no effort. It thus rose to a height much greater than that necessary to reach its nest by gliding.

CONCLUSION.

I have sincerely described, without misrepresentation, what I have seen and how I thought I saw it. The facts which I have related are, I realize, still quite incomplete, if we consider the number of these facts and of the kind of sailing birds whose performances they represent. However, such as they are, whether they be an illusion of the author or a just appreciation of facts, they seem to me to giveconfirmation to my theory.

DISCUSSION BY O. CHANUTE.*

M. Bretonnière's method of delineating the spirals of soaring birds, and of reproducing their forms by models, is exceedingly ingenious, and likely to prove very valuable in enabling us to analyze sailing flight. It is a happy thought, well carried out, and the models which he has sent to the conference well deserve all the interest which they have elicited.

* See Appendix C, p. 409, and D, p. 412.

It is to be regretted that these models cannot be exhibited to the readers of the paper, when this is published, but if those interested will take a little pains, they may easily reproduce those models for themselves. Let them procure some lead wire, of small diameter, or some soft red copper wire, annealed, and twist it into right or left-hand spirals, which, when held at the angle indicated on the drawings, shall repeat to the eye the projections of those forms as shown on figs. 8 and 9. The explanation of *M. Bretonnière* is so clear that the models can easily be made, but some little care is requisite to hold the spirals (while testing them) at the correct angle above the horizon, so as to reproduce accurately the ascending and descending portions of the course.

After some practice has been thus obtained, it will materially advance our understanding of soaring flight, if observers who are favorably circumstanced will repeat such delineations and representations in the solid of the paths of birds, and publish the results of their observations and deductions.

As to *M. Bretonnière's* theory of soaring flight, while it is not complete, it seems to me more rational than any other which has been presented to the conference, or indeed than any that I know of; and I have examined—I may say, studied—some 15 or 20 others.

Professor Langley's very ingenious and rational theory, based as it is upon what may be termed the discovery of the natural law of wind pulsations, doubtless accounts for many of the phenomena, but when quantitative figures are applied, the demonstration is weak in the case of the bird supposed to be progressing in the same direction as the wind. Moreover, this theory does not explain spiral soaring, nor why certain species of birds are sailers, and certain others of equal size can glide, but cannot soar.

I have no doubt that when an accepted theory of soaring flight is finally formulated, the pulsations of the wind shown to exist by Professor Langley will be acknowledged to be an important, perhaps an indispensable factor, but this alone, without consideration of the form of the bird and of his manœuvres in the air, either in broken or spiral motion, does not seem to me as yet to furnish a sufficient explanation and theory of the very remarkable phenomena exhibited by the sailing birds.

M. de Louvrié, in his paper, endeavors to account for spiral soaring. We need not now consider this performance with an ascending wind, as it is evident that with an ascending trend of 10 to 25 per cent., a supporting reaction and a forward impulse can easily be both figured out. We must confine ourselves, therefore, to a criticism of the theory of soaring with a horizontal wind. M. de Louvrié does not explain *how* the action of such a wind upon the body of the bird modifies the component of the normal pressure under the wings, so as to produce an advancing force during three-quarters of the circle, but he is led to assert that the bird plunges downward when progressing against the wind, which is directly disproved by

all observations hitherto made, except, perhaps, those of Prince Bakounine.

Mr. Wellington, on the other hand, supposes the bird to gather impulse *from the wind* while sailing in the same direction in which it blows. This clearly cannot be true, because the bird must sail faster than the wind in order to be supported at all by the pressure due to its speed. This recalls the theory once proposed, and now abandoned, by *M. Mouillard* and by *M. Weyher*, that the sailing bird gathered impulse from the wind, by reason of the difference in the coefficient of resistance of his front aspect and of his rear aspect, thus spinning round on a spiral like a set of Robinson's anemometer cups. This theory being disproved by the consideration above mentioned, that the bird must glide faster than the wind blows. We have also among the papers at this conference, the theory of *Mr. William Kress*, who argues that the bird gathers energy by passing from one stratum of air into another stratum blowing at greater speed, but who furnishes no evidence that air is usually stratified in that particular way. It is true that observation shows that the velocity of the wind generally increases with the altitude above the ground, but this increase is so gradual as in nowise to resemble the diagrams which are given, and it does not seem to afford sufficient power for the effect produced, even if it were in regular strata, instead of the irregular pulsations shown by the diagrams of Professor Langley.

M. Bretonnière is more rational. He supposes the bird to gather velocity *from gravity*, by plunging downward *across the wind*, and then to regain the height so lost, and more, by facing the wind and profiting by the increased pressure so produced, which, of course, would raise the bird higher than its initial altitude, but at the expense of some velocity. This recalls the theory of *M. Drzewiecki* (which was, however, less sound), which assumed the bird to gather speed from gravity by plunging downward *against the wind*, when there would be greater loss than in the course across the current; also the later illustration of *M. Mouillard*, who likens the soaring bird to a car descending and ascending the planes of a "roller-coaster" or "Montagnes Russes," while the whole roadway is supposed to travel in a contrary direction to the car, thus enabling the latter to rise higher than its point of departure.

M. Bretonnière's theory has several flaws. In the first place, he adds together the *vertical* velocity due to gravity with the *horizontal* velocity of the wind. In point of fact these two speeds must be compounded, or an allowance be made for the loss incurred in effecting the change of direction from vertical to horizontal. In the second place, he makes no sufficient account of the resistance which the body and wing edges of the bird offer to forward progress, and which constantly absorb kinetic energy. In the third place, if the theory were true just as stated, without any qualifications, then the consequence would be that *all birds could soar*, whatever their shape, while it is well known that many species cannot.

This consequence results from the fact that the reasoning has been applied to mathematical *planes*, instead of taking into account the different shapes of various birds. This may be tested by examining what would happen to *M. Bretonnière's* poplar plank, if endowed with the faculty of preserving its equilibrium, and heading against the wind at an angle of incidence above the horizon, after having obtained a certain horizontal velocity by plunging down across the wind. The kinetic energy thus acquired may evidently then be exchanged for elevation and the initial altitude be recovered, less the losses from "drift" and "head resistance;" but when this has been accomplished the initial velocity will have been used up, and the *plane* will then be in the position of being exposed, with no velocity of its own, to the action of the wind. It is clear that the wind can then raise it still higher, but from what we know of the action upon *planes*, it is also clear that it will be *blown back*, so that the increased elevation gained is obtained at the expense of distance.

It may be argued that the plane will rise at a comparatively steep angle, and descend at a much flatter angle, thus more than regaining the distance lost, but in that case, in order to have a sound theory, it is necessary to give quantitative calculations, so as to show that the gain in distance is greater than the loss, and even then it does not explain why the albatross, the frigate bird, and the vulture can soar, while the duck, the wild goose, and the wild turkey do not.

In point of fact I find great differences in the cross-section of the wings of these various birds, the non-soaring approximating more to flat planes, in which the "drift" is probably too great in proportion to the "lift" to permit the bird to sail upon the wind, and this performance seems to require peculiarly curved surfaces, which shall recover at least a part of the power expended in overcoming resistance.

In conclusion it may be said that the theory advanced by *M. Bretonnière* is rational, and may furnish a good foundation to build on, but that it is incomplete and will not be convincing until worked out so as to account for all the phenomena, and supported with the necessary quantitative calculations. A complete theory must take account of all the energies, speeds, and pressures, and also explain how the soaring bird can advance against the wind at the same time that it rises higher than its point of departure, allowing meanwhile for "hull resistance" and for "drift."

DISCUSSION BY A. F. ZAHM.

In his otherwise admirable paper on gliding flight, M. Bretonnière seems to me to affirm that a bird can soar in a horizontal wind of uniform velocity and direction. This is equivalent to saying that a bird could soar inside an indefinitely large closed car moving with uniform speed on a straight level track. And this in turn is equivalent to saying that a ball, starting from rest at a certain point inside such a car,

could roll down a properly formed groove and rise above its initial level. The latter statement can be disproved analytically, and might easily be disproved by experiment.

This oversight of M. Bretonnière's recalls a similar one committed by Mr. Wellington in treating the same problem.* He affirms quite correctly that a ball, rolling down an inclined plane toward the forward part of a ship in motion, would "take energy" from the moving ship; but fails to notice that it would give back energy to the ship in equal amount before it regained its initial level. If Professor Langley were discussing the problem he would, I dare say, contrive to have the ship slow up when the ball reached the bottom of the inclined plane, so that it might rise more nearly to the height due to its velocity. If Mr. Wellington were to assume for the ship a favorable rocking motion or pitching, it would be quite easy to demonstrate both analytically and experimentally that the ball could rise above its initial level. Some such assumption is made by M. Mouillard, in his well-considered article in the *Cosmopolitan* for February, 1894.

It may be as well to state that it is mechanically impossible for a bird to soar in an even wind or in a calm; so that, when observers profess to have witnessed such performances, they ought hardly to claim our credence unless they show by exact records the behavior of the wind throughout the entire course of the bird's flight. When we remember that a gentle swelling and falling of the parts of the atmosphere, such as would naturally occur over the billows at the surface of the sea, is sufficient to account for soaring, it will appear how important it is to consider even the slight pulsations of the air.

The complete theory of soaring might be very conveniently discussed under three distinct heads: (1) Soaring in winds of upward trend; (2) soaring in winds of variable velocity; (3) soaring in winds of variable direction. The first has been clearly presented by many writers; the second by a few; the third has not, I believe, thus far received much development, possibly for want of data. It is quite evident, however, that if the breeze veered frequently from side to side or up and down, that the bird's inertia would offer such resistances to these changes as to afford a considerable propulsive force. It is to be hoped that this phase of the soaring problem will be duly developed when more exact data are at hand exhibiting the variations in the wind's direction at different altitudes and in various localities. For it is evidently not greatly material whether the bird's wings beat the wind occasionally or the wind beat the bird's wings by occasional veering.

The first two phases of soaring have been well developed by M. Bretonnière, and his paper will contribute much to the complete elucidation of this subject. His descriptions of ascending currents, and whirlwinds especially, will be of interest to those who have not had opportunities for similar observations.

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* *Engineering News*, October 12, 1893.

A THEORY OF SAILING FLIGHT.

By William Kress, Vienna, Austria.

It is my understanding that the bird, so long as it sails in a uniform, evenly flowing current of air, moves, as regards to the surrounding medium, precisely as if it were in calm air. That is to say, that however fast the current may blow, it will influence the bird in its motion only as regards the earth ; the wind taking the bird more speedily to its destination, or retarding it as the case may be. If the bird flies with or against the current, then its own proper speed as regards the earth must be added to or deducted from the speed of the current.

Notwithstanding this compounding of velocities as regards the earth, the bird moves in a uniformly flowing current just as he would in calm air so far as its "relative wind" is concerned, and feels only the wind which results from its own speed, and which it encounters only in front.

Thus much in a uniform current ; but the wind near the earth's surface is not an evenly flowing current of air. There are streaks and waves of air of different speeds, generally increasing in velocity with the altitude. This we know from the varying sound of its whistling, and we feel it on our faces in stormy weather when at sea or at some exposed spot, and we easily realize that the wind constantly changes in speed and also partly in direction.

These differences in the speed of the wind are the factors which enable the well-known soaring birds, such as the albatross, the frigate bird, etc., to sail for hours upon the breeze without flapping their wings. Each difference in speed is skillfully used by the bird as a new source of extraneous power.

Let us represent in fig. 1 an ideal condition of the moving sea of air in which strata of wind of different speeds are vertically superposed, increasing and decreasing regularly above and below a central zone.

It will be noted that at the center, at A, the speed is 5 meters per second, and increases to 10 meters at B and C.

Let us suppose that the bird is sailing across the wind, with an initial velocity of its own of about about 15 meters per second in the slower stratum A, and then turns from e to f against the wind into the stratum B ; as the wind thus increases from 5 meters to 10 meters per second its lifting effect becomes greater, and the bird can reach, with the aid of this wind, a certain limited height without flapping his wings or changing their angle of incidence and without losing any portion of his initial velocity with regard to the surrounding medium.

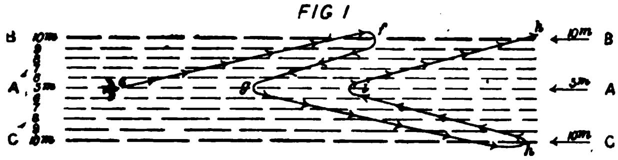
The limit at which the wind ceases to increase in speed would be also the limit of the height attainable without "work." The amount of kinetic energy or of visible speed which the bird loses in going from e to f, through air resistance and friction, is replaced by the increasing kinetic energy of the

wind—that is to say, the bird reaches a greater height without losing any of its own speed to the air.

When the bird has reached a certain height, as at *f*, it then turns and sails back in the direction of the wind to *g* into the calmer stratum of air *A*. During this second course the bird gains acceleration of its own velocity by gliding downward upon an oblique surface of air, and, moreover, it carries part of the kinetic energy due to the greater speed of the stratum *B* into the stratum *A*, which constitutes another gain. Therefore, if everything is favorable, the bird can return into the original stratum *A* with greater speed of its own than it had at the start at *e*, although it has traversed the course *e*, *f*, *g* without flapping its wings—that is to say, without "work."

In like manner, with the arrangement of wind strata illustrated in fig. 1, the bird can sail over the course *g*, *h*, *i*, *k* with no loss of energy or of speed.

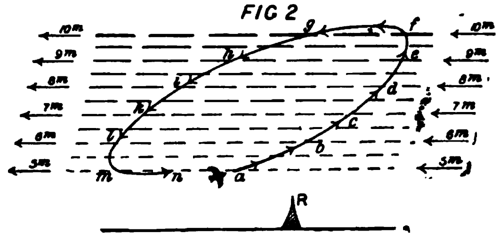
FIG 1



It must be particularly noticed that the more rapid currents of air need not necessarily be above each other, as in the case first supposed, but that they can just as well be downward, or sideways, or in any direction whatever; the only requisite is that the bird, when it passes from the calmer into the faster current, shall sail against the latter, or *vice-versâ*, when it passes from the quicker into the slower stratum shall sail down with the wind.

In both cases the difference in speed of the currents of air is always to the bird an extraneous source of kinetic energy,

FIG 2



which it would lose almost immediately if it were to remain longer in the same stratum of air—that is to say, in a current of uniform, even velocity.

Let us now show how circling flight of birds of prey can be explained in a wind gradually increasing in velocity upward, this being the usual condition and being represented in fig. 2.

Let *R* be a fixed point on the earth. At the height *m, m, o, a* the speed of air in reference to *R* is 5 meters a second; at *f, g* it is 10 meters a second in the direction shown by the arrows.

The bird's own velocity, with respect to the surrounding medium, may be 15 meters per second.

Its course and its speed, with reference to the fixed point *R*, is shown at each of the points lettered in the diagram by the following table:

VELOCITIES.		*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*k*	*l*	*m*	*n*
Speed of wind	v	5	6	7	8	9	10	10	9	8	7	6	5	5
Speed of bird	v'	10	9	8	7	6	5	24	24	24	24	24	24	13
Relative speed	v''	15	15	15	15	15	15	14	15	16	17	18	19	18

This tabular statement shows that the bird has completed his circling course from *a* to *b, c, d, e, f*, and back to *m, n*, or to the point from which it started, without once flapping its wings—*i.e.*, without muscular effort, and has at the same time gained in speed 3 meters per second.

From *m* to *n, a*, etc., to *f*, $v'' = v + v'$, and from *f* to *g, h*, etc., to *m*, $v'' = v' - v$, as shown in the table.

Therefore the differences in the speed of the wind are the inexhaustible sources of power utilized in sailing flight which we so greatly admire when performed by the albatross, the frigate bird, the vultures and other expert soaring birds.

I have treated this question more in detail in a paper upon "Wind and Sailing Flight," published in No. 9 of *Zeitschrift für Luftschiffahrt* for 1888.

SOARING FLIGHT.

BY E. C. HUFFAKER, C.E.

BELOW are given the results of two years of careful observation of the flight of vultures in the valley of East Tennessee, where the birds are both numerous and active. In connection with the observations, the condition of the weather, the presence or absence of winds, the topography of the country, and such other conditions as might be supposed to modify flight have been carefully considered. Except in cases of extraordinary flights, which will be given separately, each feature described has been verified by hundreds of observations.

The valley of East Tennessee lies in the region of the counter trade winds, is bounded on the southeast by the various ranges of the Alleghany Mountains, and on the northwest by the Cumberlands. It is intersected by countless ridges and

hills, rising in many cases to a height of 500 ft. above the general surface of the country. During the months of February and March winds and storms are common, while during the months of summer and autumn winds are the exception rather than the rule. It will, therefore, be convenient to consider the subject under two divisions: (1) Flight in windy weather, and (2) flight in the absence of winds.

I.—FLIGHT IN WINDY WEATHER.

1. In windy weather, especially if the wind be strong, the vulture very rarely flaps its wings.

2. If the velocity of the wind be greater than 30 miles per hour the vulture cannot make direct headway against it, but must resort to what may be termed a system of tacking, the bird alternately rising and falling, and so working its way slowly forward.

3. In such a wind the bird will often stand motionless for several seconds, neither advancing nor receding. However, an occasional slight movement of the wings can usually be detected. This feat is commonly performed upon the brow or slope of a hill facing the wind; though it not infrequently occurs in comparatively open fields of level land.

4. The bird, having become stationary, may rise to a height of 15 or 20 ft. by drifting slightly backward, the under surface of the wings being exposed to the current. The backward movement is arrested by bringing the wings into a horizontal position, accompanied by subsequent slight movements. It may then advance by gliding downward to a point several feet directly above its former position. The manœuver may then be repeated. The bird may rise perpendicular (often very rapidly) to a height of a hundred feet or more (this usually takes place upon the brow of a hill), or it may advance into the wind while rising, the manœuver being accompanied by a rocking movement.

5. By heading across the wind, a horizontal or slowly descending flight of great rapidity may be attained, the bird covering a distance of a quarter of a mile in an incredibly short time. In such flight the bird never heads in the direction of its course, but makes a slight angle with the wind, so that if a bird be moving upon a tangent it is possible to determine in this way whether or not the wind is blowing and with what velocity. This swift flight may be suddenly checked by turning and facing the wind.

6. When, having become stationary, the bird turns to fly with the wind its path is an approximate cycloid, the bird becoming stationary at the cusps. In advancing, however, to its former position it follows no regular curve, having to beat its way against the wind.

7. In a strong wind the bird does not attempt to soar by circling; but if the wind be moderate such flight is not uncommon. A flock of vultures rising from a carcass in such a wind will usually soon be found drifting with the wind as they soar,

as if the entire flock were blown forward by the wind. Their object being to gain elevation, they make no effort to contend against the wind. Such flight in a horizontal plane is in epicycloidal curves.

8. The black vulture can advance into a wind against which the buzzard can make no direct headway.

9. When sailing with the wind flight is invariably more rapid than when advancing, so that no uniform velocity is to be looked for when the bird is circling in a wind. In other words, if the velocity be not uniform in a horizontal plane the bird cannot close the circuits.

10. Among the rare flights that I have witnessed I may mention that of a flock of vultures hovering about the brow of a hill in the face of a wind, which moved off simultaneously with all apparent ease in the direction from whence the wind came to hover again over an open country a mile away. That of a buzzard descending the slope of a hill 1,500 ft. with a fall of perhaps 300 ft. and at once turning and retracing its course to the top of the hill. I could not determine whether the wind was blowing across the hill or not. The flight was very rapid, and the ascent made with a vigorous rocking movement along a sinuous course. Also that of another descending across a valley in a light wind at an angle of 30°, a distance of 500 ft., to retrace its course with the same powerful rocking movements.

II.—FLIGHT IN THE ABSENCE OF WINDS.

1. Flapping is much more frequent in the absence of winds. The occasions on which flapping is resorted to are when first rising from the ground, when near the earth, when alarmed, when journeying alone, when seeking their roosting place in flocks, in which case gliding flight alternates with flapping flight.

2. When the bird begins soaring in circles flapping usually ceases; this is especially true of the hawks.

3. The buzzard, the black vulture and the hawks can soar indefinitely in a dead calm. As a special instance of what I have many times witnessed I may mention the following: About nine o'clock in the morning, on a bright day in June a buzzard began soaring immediately overhead at an elevation of about 50 ft. near the corner of a level, open field, bounded on two sides by woodlands in which the trees rose to a greater height than that of the buzzard. Not a breeze stirred among the leaves, and there was no indication of any surface current. The flight was elliptical and very uniform, the bird returning upon its course after each revolution. Its velocity was about 25 ft. per second, and it required about 5 seconds to complete a revolution, its path being about 125 ft. in length. It made no less than 20 revolutions in an approximately horizontal plane; so that in about 1 minute and 40 seconds it had flown a distance of 500 ft., had maintained both its original elevation and velocity, and had not made one beat of its wings. Having completed its inspection, it began rising in circles, still

without flapping. After reaching a height of 200 ft. it sailed away on a tangent. The heating of the earth might have produced a slight rising current; but if so there was no corresponding surface current from the woods, and it is certain that such ascending current, if it existed, would not have sufficed to float thistle-down.

4. In gliding flight the bird sails on rigid wings, descending at the rate of about 1 in 10, and traversing long distances. The lost elevation is regained by soaring.

5. As a rule the buzzard either flaps or soars or glides downward toward the earth. The instance in which he glides for long distances horizontally are exceedingly rare.

6. None of the birds mentioned soar in a calm on rigid wings. In the case given above the bird was continually changing the angle of its wings. It was soaring to the left, and the tips of right wing were sometimes 18 in. above those of the left; and again both wings for a moment were horizontal. Each horizontal position was followed by a marked depression of the left and elevation of the right. Each of these movements was equivalent to a slow and measured stroke of the wing, and as two or three occurred during each revolution, the bird was in reality continually beating its wings.

7. In soaring, velocity is gained at those points where the curvature of the course is increasing. In the above case of horizontal soaring, where the velocity was very nearly uniform, a slight increase could be detected as the bird passed the vertices of the ellipse. In the more common mode of flight, however, the velocity is not uniform. Instead of moving in horizontal circles the bird consumes such energy as it may possess by gliding upward. Having lost its velocity, it hangs for a moment almost motionless, turns backward, sweeps suddenly downward like a wheel with the outer wing greatly elevated, then with a single powerful stroke brings the wings and body into a horizontal position and speeds away with a velocity due to a fall of twice as many feet as it has taken. All these movements require the expenditure of energy; and that the bird exerts great energy is indicated by the fact that it requires a short period of rest in horizontal soaring before the manœuvre is repeated. And even in those remarkable ascents along helical courses in which the bird climbs so rapidly the principle of rapid movement upon sharp curves still holds good. These flights are so rare that it is difficult to decide whether or not they are accomplished by the aid of ascending currents. But they occur most frequently in sheltered spots, and I have never witnessed one on a windy day.

8. It is obviously important, in endeavoring to determine whether or not a bird can soar in a dead calm, to keep in view the bird's intention while soaring. The only currents which, near the earth, there is difficulty in recognizing are vertical ascending currents. Does the bird then rise by soaring only when it comes in contact with such currents? It would seem otherwise, for he is able to pause in his flight on a calm day at any point and to soar by circling for an indefinite length of

time. The argument is that any effective horizontal or slightly ascending current could easily be detected by an observer, and that the ease which birds soar upon a calm day and their ability to do so at any point do not justify us in assuming the existence of undetected currents. And that soaring flight is to be accounted for by reference to certain well-defined and continually repeated movements which are inseparable on a calm day from such flight.

9. It is not only true that the buzzard and black vulture rarely flap their wings on a calm day when soaring at a great elevation, where undetected currents may exist, but they continue to soar when, as they often do, they return to the earth and fly near the surface.

10. In soaring at a high altitude the buzzard may often be seen to tip the primary quills downward and backward. This is invariably the beginning of a rapid downward flight.

11. In "journeying flight" at high altitudes the V form of the wings is very common, the flight being comparatively slow and descending. The same form also appears in circling flight when the bird glides upward, the wings being brought into a horizontal position as the ascent ceases. The bow form, with wings projecting far in advance of the head, is common at great altitudes; it is characteristic of slow flight. The wings in this case are horizontal. In swift gliding flight the wings are horizontal, the line of their front margins straight, the center of gravity lying but little below the center of horizontal pressure upon the wings.

12. The rowing quills are long and powerful in the buzzard, less so in the carrion crow and the hawks. These quills, which have a twisted form when at rest and a sharp downward curvature are, in flight, bowed upward into a quadrant, their twisted form giving place to the ribbon form. This is not due to any inflowing or upward current, but to the action of the quill under horizontal pressure. They are doubtless very effective in the rocking movements of the bird.

13. The tail is rarely spread or used by the buzzard even in windy weather. But in the carrion crow it is spread frequently. In the hawks it is long and used when following their prey.

14. Neither in the hawks, buzzards nor black vultures is there any upward bending of the tips of the feathers along the rear margin even in soaring flight. As seen by the reflection of sunlight from the under surface, the wing is concave throughout. This is found to be true when the bird is viewed through a telescope. In rapid gliding flight there occasionally appears to be a slight reversed curve along the rear margin of the secondaries. But in ordinary flight there is perhaps but little pressure exerted upon the rear margins.

15. The rear margins follow close upon the path of those in front, the tips of the secondaries being more elevated than the primaries, not including the rowing quills.

16. When viewed from above, as when standing upon a cliff, the upper broad expanse of the wing will invariably be found pointing backward and downward even in soaring flight, being

seldom or never horizontal. This fact adds greatly to the difficulty of explaining flight upon any known hypothesis.

17. Any fore-and-aft section of either the wings or the rowing quills will be found sharply pointed in front. This is true of all birds that have come under my notice, nature having taken special pains in marking the line of separation by the upper and lower stream lines. The front of the wing is never rounded, but is brought to a sharp angle in front. In front of the elbow three or four feathers are found which serve no other purpose than to change the rounded into a pointed form. So universal is this form that it can hardly be safely ignored in attempting artificial flight.

18. As a consequence, all the currents that impinge upon the front margin of the wing are deflected upward. The reverse form has been advocated by certain inventors.

19. The under surface of the wing is hollow, and the air, on passing the front margin, must rise to fill it or else a dead area will be formed. The coverts of the secondaries prevent this, and the entire area is swept by the currents. The depth of the concavity, which is greatest 1 in. behind the front margin in the buzzard is about 0.5 in. upon the secondaries. Upon the primaries it is perhaps not over 0.2 in. Thus both upon the upper and lower surfaces the air must rise as it meets the wing.

20. Upon the upper surface the currents produced (no reference is here intended to the presence or absence of winds) curve with the wing, never escaping as jets into the air above except perhaps as the bird alights.

21. The reaction of the air under impact is far greater than any demand of ordinary flight. Thus I have seen a buzzard swoop downward almost vertically in its attack upon another, falling a distance of 40 ft. and adding to its velocity by a powerful twisting movement, turn upon a curve of 15 ft. radius, rise almost vertically to its original elevation and sail on with its original velocity. Assuming that the original velocity was 25 miles per hour, the additional speed due to a fall of 40 ft. would be nearly 15 miles per hour, thus producing a velocity of 40 miles per hour, at which the normal pressure would be 8 lbs. to the square foot, thus enabling the bird to "ricochet" upon the air and to spring or rebound back to its original height with little loss of its original velocity.

22. When the wings of the buzzard are extended, as in downward gliding flight, the line of the front margin being straight, the center of gravity coincides very nearly with the center of magnitude, not including the tail, but slightly in front of it. The flight is in this case rapid. But it becomes far more rapid, approaching at times 80 ft. per second, when the primaries are drawn backward and slightly elevated. If, on the contrary, the wings are drawn forward, the flight is slow.

23. The most rapid flapping flight does not exceed 30 ft. per second. Flapping flight is directed upward, never downward, and occasionally accompanies circling flight. The velocity in horizontal soaring is from 20 to 30 ft. per second. Sailing

flight for short distances about the same. The crow sails twice as rapidly. The black vulture accomplishes a variety of manœuvres, with wide extremes of velocity, but is, upon the whole, much swifter than the buzzard. The wings are flapped oftener, though it may frequently be seen soaring for hours on a calm day without flapping. The stroke of the wing is rapid and powerful.

24. The purpose of soaring is twofold—to enable the bird to inspect a given locality for an indefinite length of time, and to gain the altitude necessary for extended gliding flight. In the one case the flight is approximately horizontal, in the other ascending. As a rule the hawks climb much higher than the buzzards, continuing to rise almost perpendicularly (if the day be fair) to an elevation so great that the largest squirrel hawk appears but a speck in the blue distance, and if lost sight of can with difficulty be found again. Their gliding flight extends over many miles.

25. The gliding flight of the buzzard is very irregular, much more so than that of either the black vulture or the hawks, and this too despite the fact that they glide more frequently than either of the other birds. It is the buzzard's usual mode of descent. The explanation seems to be that the buzzard in gliding flies automatically, so that any increase of velocity resulting in an upward movement is not guarded against. His flight is therefore undulating.

26. A flock of vultures, when frightened from a carcass, will rise in a body by soaring to an elevation of 200 or 300 ft. or more, when, by a simultaneous movement they will be found gliding toward the four quarters of the heavens, no two going together. Their object is the better to scour the country. They detect their prey either by scent or by sight.

27. In horizontal soaring flight on a calm day the tips of the wings are elevated and depressed on an average of perhaps about 1 ft. per second. If we allow that the resistance encountered amounts to 5 lbs., the weight of the bird (and it cannot be greater than this, else the body of the bird would be sensibly elevated) we have for the power expended 5 foot-pounds per second, or $\frac{1}{110}$ of 1 H.P. As a large part of the weight is sustained by the secondaries, which move but a few inches, the power expended is perhaps much less than this. It is a mistake to argue, from the enormous development of the pectoral muscles, that the power required in flight is correspondingly great. The great development is the result of constant use, and is by no means a necessary condition of flight. I once shot a large blue-tailed hawk, which was so lean that the pectoral muscles had almost disappeared, yet it mounted upward with the greatest ease.

28. The explanation of soaring flight involves the solution of several difficult problems, not the least of which is the manner in which circling horizontal flight is maintained. Assuming that the bird moves upon the surface of a cone having an obtuse angle, the centrifugal force would tend to preserve the angle. But allowing a complete circuit to be 125 ft. in length,

and the expanse of wing 6 ft., the tips of the outer wing must travel very nearly 38 ft. farther than those of the inner wing. By what means is this accomplished? Since the outer wing has the higher velocity, the resistance encountered should be greater. While if we suppose the inner wing to encounter greater resistance by an increase of the angle of the primaries, why is the wing not elevated as well as extended? When the bird turns from a tangent on to a curve a perceptible interval of time usually elapses between the elevation of the outer wing and the turning upon the curve, and on one occasion, while standing among some cedars, a buzzard in passing low overhead stopped to reconnoiter, and in doing so elevated the right wing slightly and turned almost completely upon an axis before wheeling to the left, so that for an instant it seemed to be sailing backward. There was no wind stirring, and the bird retraced its course with no loss of elevation or velocity.

29. In all the wing movements involved in soaring the broad surface of the wing is kept approximately in the line of flight; but it is very difficult to decide when the rear margin lies within and when without the surface traced by the front margin. To the eye, however, it appears that if the front margin were removed beyond the line of highest curvature, the remaining portion would form a broad surface, slightly concave, which would uniformly intercept the air upon the under surface. The wing stroke of the buzzard is vertical, ending with a rowing movement of the primaries, and so powerful as to elevate the body several inches (as I have witnessed in the case of a tame buzzard flying directly from me). The reaction must therefore be greater than the weight of the bird. However, during the upward stroke much of the elevation gained is lost. The stroke of the black vulture is downward, sharply forward, and very rapid. In soaring or gliding its wings are never V-shaped. The same vigorous movements characterize its soaring stroke. The wing stroke of the crow is elliptical, with a rowing movement. It is expert at gliding, but cannot soar.

30. During the flapping stroke the air is driven backward with great force, the current extending at times as far as 20 ft., as I have learned from the tame buzzard referred to. The reaction drives the bird forward. A similar reaction may be supposed to characterize the soaring stroke.

31. Among rare flights may be mentioned that of a buzzard rising out of a cove on the north slope of the Holston Mountain at about nine o'clock on a September morning. A party of us were ascending from the valley, and had stopped to rest. The bird ascended along an enormous spiral course, repeatedly plunging downward to rise with a twisting movement to a greater height. He continued to soar for some three minutes, rising 500 ft. and passing across the mountain. He made no flapping strokes, and we had noticed no wind in ascending. Nor is it probable that any ascending current existed, as the current usually begins to blow down the mountain at this hour on a warm day. That of a buzzard, rising along a corkscrew

80 ft. in diameter along the face of a perpendicular cliff; afternoon, locality in shadow, no wind stirring. That of a buzzard, gliding a distance of 1000 ft. across a valley, horizontally, with no movement of any kind, weather fair, no wind near the ground, elevation of bird 800 ft. above the valley.

EXPERIMENT.

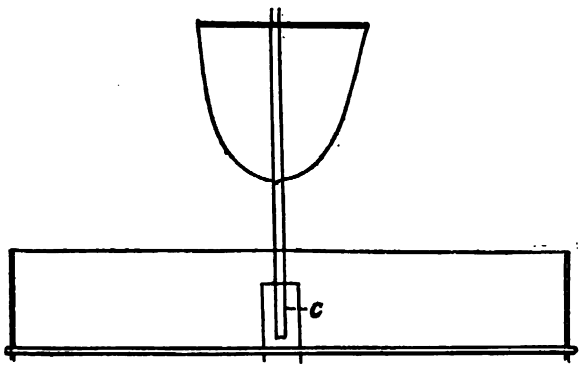
In order, if possible, to reproduce the flight of the soaring birds artificially, I have constructed a small model, shown on page 217. This model consists of a spar of light red cedar 3 ft. in length, and originally 1 in. × ¾ in. in cross section, but beveled upon the upper front margin and hollowed out underneath to resemble the front margin of the wing of a bird. Two umbrella stays 8 in. in length pass through the extremity of the spar, and are slightly curved to correspond to the curvature of a wing. Their free extremities are connected by a violin string tightly drawn. The whole is covered with black satin, nowhere tightly drawn except near the spar. The wing thus formed is concave beneath and convex above, the curvature being greater near the leading edge, and is attached at its middle point to a block of oak 2 in. × 2 in. × 5 in. loaded with lead. A rudder made of light white pine, and supporting by means of a wire a tightly drawn sail of 1 sq. ft. of surface, is attached to the block of oak in such a way that the plane of the rudder exactly coincides with the plane of the violin string and the leading lower edge of the spar.

The center of gravity lies within the block of oak midway of the wing, the rudder extending 2 ft. behind the wing. The surface of the wing amounts to 2 sq. ft. The model, when loaded with 1½ lbs. of lead, weighs 2 lbs.

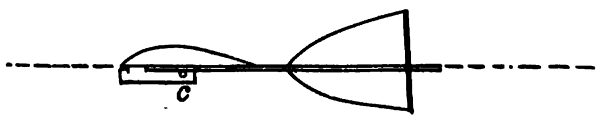
The results accomplished with this rough model are in many respects remarkable. When lightly thrust from the hand it sails away in a truly straight line, and with an easy and steady flight in striking contrast with that of an inclined plane similarly mounted. The flight is in all respects similar to that of the soaring birds when descending along a gradient of 1 in 15. The velocity attained has usually been about 25 ft. per second, or 17 miles an hour. I have obtained short horizontal flights with a smaller velocity. If thrust rapidly forward the model rises to a height of 8 or 10 ft., when, hanging suspended for a moment, it sails forward to the ground. Having attached a vertical rudder, I exposed it to the mercy of a high and boisterous wind. Under the action of a strong and steady breeze it rises to the height of 12 or 15 ft., slowly retreating from the wind, but always facing it. Having reached a certain height it invariably begins a descent into the face of the wind. This action is precisely that of the soaring birds in facing a strong wind; and it has a most important bearing upon the question of artificial flight, showing as it does that an air ship may be so constructed as to rise in a high wind and return to the spot from which it arose without danger to an aeronaut. Even

in an irregular and stormy wind the model is not overturned, but faces about quickly under the action of the rudders.

I next tried the effect of exposing the model to the action of a steady wind blowing at the rate of perhaps 30 miles an hour, or 45 ft. per second; at the same time keeping it stationary by holding it at arm's length overhead. Two fingers were laid directly above the center of gravity, so that the action of the horizontal rudder kept the wing always parallel to the current. Not only did the wind support the weight of the model so that it rocked easily back and forth beneath the fingers, but when the wind was high it produced an upward pull which could not have been less than 2 lbs. additional. So that, with a



PLAN



VERTICAL SECTION

velocity of certainly not more than 35 miles an hour, the model supported time and again a weight of 4 lbs., or 2 lbs. to each square foot of sustaining surface—not counting the rudder, which had only its own small weight to support. Nor did this upward pull occur momentarily, but was strongest when the wind was steadiest and the model stationary.

While nothing better has, I think, been recorded of the lifting power of a model, the small amount of power required to hold it against the wind was not less remarkable. So slight was it that the model rocked back and forth easily, and the resistance apparently decreased with the velocity of the wind.

I repeatedly tried the effect of pulling the model downward, and in every instance it was driven forward into the wind, reproducing again the effect of the slight wing movements of the bird.

Success with such a model depends largely upon the concentration of weight near the center of gravity, with the employment of very light wings and rudders. But, with the materials at my disposal, I have not been able to construct a model capable of testing thoroughly the principles involved. A few pieces of fish-hook steel wire well tempered are all that is necessary to the making of a suitable model. But these I have not as yet succeeded in obtaining.

DEDUCTIONS.

History has shown that it is unsafe for a philosopher to sit in his study and work out the laws of nature. I shall therefore not attempt to combat the position of those who hold that soaring flight cannot be accomplished in a dead calm, but assuming that the feat is possible, shall close this article with the following deductions from the observations recorded above:

1. Flight, whether soaring or flapping, finds its basis in the expenditure of muscular energy.

2. Except as regards rapidity and extent, the soaring stroke is very similar to the flapping stroke.

3. The soaring strokes may be made by either or both wings. On an average one soaring stroke is made each second, with an extreme sweep of from 1 to 3 ft. In horizontal soaring the strokes are made with a good degree of regularity. In ascending the stroke is much more powerful and less regular. In flapping flight about two full strokes are made each second by the buzzard.

4. These strokes have for their object propulsion and elevation, not guidance. As in the flapping, so in the soaring stroke, the depression of the wing produces a backward current with forward reaction.

5. The reaction of the flapping stroke is greater than the weight of the bird, which is elevated some inches bodily at the same time that it is propelled forward. The reaction of the soaring stroke is but little greater than the weight, and results not in elevation, but in propulsion.

6. The rate of ascent by the flapping stroke is ordinarily (for the buzzard) about 6 or 8 ft. per second. The soaring stroke results in horizontal flight, or, if more pronounced, in an average rate of ascent of less than 1 ft. per second.

7. The soaring stroke is more economical of energy than the flapping stroke, there being no loss of elevation during the up stroke. In the flapping stroke of the black vulture this loss is very small; partly on account of the rapidity of the strokes, partly upon the character of the stroke. Unlike the buzzard, its stroke is not utilized directly in ascent, but in propulsion. The same vigorous beat may be recognized in both the soaring and flapping stroke.

8. It is not necessary that we should be able fully to explain the manner in which a slow, measured stroke is made effective in rapid flight, for we are still in doubt as to the philosophy of the flapping stroke. The all-important question is, Are such strokes effective? That they are the birds themselves are daily testifying. Practically the fact may be of vital importance. In estimating the energy expended we are no longer under the necessity of introducing the velocity of the flying machine as a factor in our calculations—a factor which quickly consumes the power of the strongest motor. Instead we have only to multiply the vertical sweep of the soaring stroke by the weight of the machine.

9. To show that this assumption is not unscientific, let us consider an illustration. Let us suppose that a machine fashioned like a bird is gliding over the ice, only the tips of the wings touching the ice. Any force applied internally which would, in the air, depress the wings, will now elevate the body of the machine, and the power required will be altogether independent of the velocity of the machine. Suppose the machine has been thus elevated 2 ft., and that the ice suddenly inclines upward a distance of 1 ft. If the wings are light and allowed now to rise freely, they may make the ascent of 1 ft. before the body of the machine has fallen 1 ft, and thus the entire machine is raised a distance of 1 ft., and the movement may be repeated indefinitely. In flight the case is similar; since the air over which the bird is gliding is sufficient for its support, it may use that support in adding to its elevation.

10. There is, however, this difference: the stroke in the air may add to the velocity instead of to the elevation. This point is less clear than the other. But it may be noted that when a downward stroke is made the pressure which is everywhere increased is made effective upon the under front margin (the sharply curving portion) by the yielding of the rear surfaces. The air which would otherwise escape in all directions is caught in the hollow of the front margin, and by its reaction the bird is driven forward. For it is to be remembered that while the bird is moving but 80 ft. per second, the increased pressure is translated at no less a velocity than 1,300 ft. per second. The downward pressure of the wing is therefore felt instantaneously upon the front margin, and this pressure giving way at all other points except above and in front, the bird is both sustained and driven forward.

11. We see, therefore, why Nature has taken such pains in fashioning the hollow front of the wing. She has made wings broad and narrow, large and small, long and short, curved in various direction and adapted them in a hundred different ways to the necessities of specific flight. But in the design of the hollow margin in front she has never made a single variation. Even in the fashioning of the rowing quills, she has not departed in one instance from her universal method.

THEORY OF SOARING FLIGHT.

BY CH. DE LOUVRIÉ, ENGINEER, FRANCE.

SOARING flight, or flight without beat of wing, which has also been called *sailing flight* (*vol à voile*) because it can be performed only in time of wind, has been thoroughly observed and well attested, and yet its possibility is denied by those who cannot explain it, and who attribute the bird's support to some sort of *vibration* of the wings, or, in other words, to an *imperceptible* beat of wing.

In point of fact, it cannot be explained by the old formula, taught in all the schools, that oblique air pressures are in the ratio of the square of the sine of incidence; but it is very clearly expounded with the aid of the formula which I proposed in the *Revue de l'Aéronautique* (fourth issue of 1890), supplemented by the law which I deduced from my experiments in 1866 (analogous to those of Professor Langley), that the formula $R = W\ tang.\ @$ expresses the relation which the weight W sustained by a plane maintains to the resistance R which the plane opposes to horizontal motion at all angles of incidence, @. This formula demonstrates that the weight W and the resistance R are the two rectangular components of the air pressure, which is normal to the plane and not in the direction of the wind.

To fully satisfy my mind in this matter required but a ten minutes' demonstration furnished by two kestrel hawks at Mount Valerien in 1866. We were upon the slope which overlooks St. Germain, from which a good wind was blowing at the rate of about 40 ft. per second. Some 20 paces in front of us two kestrels were floating about 60 ft. above the earth, and heading against the wind. At times they remained immovable, and again they would slowly rise or descend, or gently advance or recede, or again glide sidewise. Their wings remained immovable. All at once they rose abruptly and drifted back, and then performed the same manœuvres at a height of 65 ft. above the fort, but we then remarked that the equilibrium seemed more unstable; and Mr. d'Erterno, the judicious and persevering observer, said to me: "I wish the French Academy was here; it would no longer deny sailing flight."

Evidently the current of air flowed parallel with the inclined soil, and therefore the wind was ascending. The plane of the kestrels' wings might then be inclined at an angle pointing below the horizon, and the angle of impact of the wind would remain sufficient to furnish support.

Now, if the plane points below the horizon, the normal pressure and the action of gravity (weight) are no longer opposed on the same straight line, and there is a resultant in the direction of the inclination which drags the body forward. Thus, with an ascending wind the sailing bird may proceed as he wills; it is sufficient for him to incline the plane of his wings

toward the side to which he proposes to move, or he may, if he prefers, remain immovable.

It is a somewhat more difficult feat to perform with a horizontal wind, and the sailing bird must then resort to a very simple artifice, which I am about to explain.

Doubtless the bird must move in the direction resulting from the normal pressure under the plane of the wings; but the action of the wind upon the *body* of the bird creates a component which, if it continued too long, would drift the bird back, and which modifies the direction of the normal pressure. Now the bird's inertia opposes this, employing it to gain as much height as possible in order to regain later, by gravity, any possible drift or lost distance.

For this purpose he sweeps around in those orbits so often described in order to change constantly the direction and to evade this component. Indeed, we know that the quantity of motion $M\ V$ imparted by such a force is proportional to its time of action, t, and that $M\ V = F\ t$. The bird may thus traverse about three-quarters of the circle almost without drifting; but during the fourth quarter the wind is adverse, and in order to sweep around to advance he must sacrifice support from the wind. Accordingly he inclines his wings some degrees below the horizon and plunges into the current; his weight propels him and imparts a kinetic energy which is subsequently expended by inclining the plane of the wings upward again. The wind then raises him up again to the height of departure, and he profits by all the altitude gained in the three-quarters of the circuit; and the stronger the wind the greater the height gained. By this device, which is imitated in the "roller coaster" sport, the sailing bird can advance against the strongest currents without sensibly losing its altitude.

In order to sail across the current it is sufficient for the bird to incline his wings in the proposed direction, making at the same time the least possible angle with the wind. Now, this procedure is possible around three-quarters of the circle, and we have shown how he can advance against a head wind by vertical tacking during the fourth quarter circle.

The sailing bird may therefore progress in any direction whence-ever the wind may blow, providing it be sufficiently strong. What now is the minimum necessary wind? The formula which we have given will answer this question. The answer necessarily depends upon the angle of incidence @ and upon the ratio of the wing surface to the weight of the sailing creature.

If we call S the wing surface, equal to 1.22 sq. ft. per pound (1 square meter to 4 kilograms), this being the usual ratio with sailing birds, and we call V the velocity of the wind, W being the weight, then for mere support we must have the equation:

$$W. = K.\ S\ V^2 \left(\frac{2\ sin.\ @\ (1 + cos.\ @)}{1 + sin.\ @ + cos.\ @}\right) cos.\ @,$$

and if we make @ $= 2^\circ$ and $K = 0.005$, we have:

$$1 \text{ lb.} = 0.005 \times 1.22 \times V^2 \left(\frac{2 \, sin. \, 2^\circ \, (1 + cos. \, 2^\circ)}{1 + sin. \, 2^\circ + cos. \, 2^\circ}\right) cos. \, 2^\circ,$$

and hence

$$V = \sqrt{\frac{1 \times (1 + sin. \, 2^\circ + cos. \, 2^\circ)}{0.005 \times 1.22 \times 2 \, sin. \, 2^\circ \times (1 + cos. \, 2^\circ) \, cos. \, 2^\circ}} = 49$$

miles per hour. Or, if we make @ $= 5^\circ$, we have $V = 12$ miles per hour.

These two examples exhibit the importance of the angle of incidence @, which serves to regulate the effect of the wind. With an ascending wind this angle may be large, and thus soaring flight becomes possible with a light breeze.

With the old formula that the normal pressure varies as $sin.^2$ @, one obtained for an angle of incidence of 2° a required velocity, $V = 383$ miles per hour, and for an angle of 5° $V = 156$ miles per hour, so that sailing flight seemed impossible unless the wind was vertical. It could not be thus explained; and we see that the formula for normal pressures under oblique planes is the very foundation-stone for an explanation of sailing flight as well as for aviation in general, and that it cannot be correctly expressed by a fanci ul formula such as that given by Commandant Renard to the Société de Physique in 1889—viz., $N = K \, S \, V^2 \, (A \, sin. \, @ - (A - 1) \, sin.^2 \, @)$, in which A is a coefficient, *larger than* 1 *and probably equal to* 2. (!)

But, in point of fact, what is the general trend of the wind? We need but watch the whirling of dried leaves in late autumn to realize that such a thing as a horizontal wind hardly exists, except perhaps in the upper regions of the atmosphere, and a little consideration suffices to show that it cannot be otherwise. Thus, if the current encounters a hill it is deflected and flows upward to a great height by reason of the kinetic energy and expansibility of its molecules, while on the reverse slope the air remains undisturbed, and one can barely perceive a slight suction. Let the wind encounter a gorge and it pours into it, following all its irregularities, and inasmuch as the molecules can no longer preserve the same velocity, whirls and eddies are produced which mount to a height proportioned to the strength of the wind. Moreover, the wind advances in great waves, and near the equator the heat of the sun actually produces vertical winds.

The sailing birds profit by these currents to rise, because acquired altitude represents so much of stored energy, which can be subsequently expended, and because the wind acquires speed with elevation, as so well shown by the observations made in 1889 at the base and summit of the Eiffel Tower. Once altitude attained, gravity may be availed of to compensate the intermittence in the wind; and the albatross may, far from any danger, go to sleep amid the clouds.

Thus sailing flight is easy both to understand and to imitate; for the soaring bird can give to his aeroplane all desired inclinations, using his tail as a rudder. If his neck be long he can produce the same results by changes in his center of gravity through movements of the head, to the right or to the left, forward or backward, as the case may be.

It is therefore seen that sailing flight may be reproduced by man. It complements rowing flight, and it is important to be able to effect both with the same apparatus in order to obtain both economy of power and safety in time of storm. This I have endeavored to effect in designing my "Anthropornis," with which man will be no longer the sport of the wind, but, like the eagle, be the king of the air.

DISCUSSION ON THE VARIOUS PAPERS ON SOARING FLIGHT

BY J. J. MONTGOMERY, OF SAN DIEGO.

About six years ago I discontinued temporarily a course of study on the flight of birds and the laws relating thereto, after having pursued it for a number of years in a rather isolated place. From my investigations and experiments I have been able to draw some conclusions which agree well with some presented in papers read at the conference concerning the soaring of birds and the nature of wing surfaces.

After many experiments with kites, I became convinced that the laws of incidence and reflection, as ordinarily applied to planes, did not apply in the case of a fluid medium impinging on a plane surface, particularly when striking it obliquely; and I commenced a series of investigations to discover what are the movements of fluid particles when approaching plane surfaces, and the consequent actions and reactions.

For this purpose I pursued three sets of experiments.

In the first, the movements of the wind when striking a plane were studied; in the second, the movements of a gentle draft against a small plane; and in the third, the movements of water, when striking a plane, in a slow current.

In the first experiments very light down was scattered in a breeze moving from 5 to 10 miles an hour. When free from disturbances it traveled in straight lines, but when an obstruction was placed in its path it changed its direction and movements in various ways, according to the nature or position of the obstruction or the velocity of movement.

Perfectly plane surfaces were used, the largest being 8 ft. high and 4 ft. wide, standing perpendicularly on the ground, and so arranged that it could be adjusted at various angles to the direction of the wind.

When it was so placed as to cut the wind edgewise, there was no appreciable disturbance of the floating particles of down, but on giving it an angle, ever so slight, the particles approaching it changed their direction of motion, tending to strike the surface less obliquely.

This tendency manifested itself near the front edge when the angle was very small, but as the angle was increased the

particles changed their direction at greater distances in advance of the plane. For angles between 5° and 20° the deflection was noticeable at distances varying from two to six times the width of the surface (meaning by width the distance from the front to the rear edge). The deflection, very slight at first, became more marked as the plane was approached ; and in gentle velocities, the particles striking the forward part of the surface, traveled in lines nearly normal to it. In some instances the particles, striking the plane a short distance back from the front edge, would turn, then advance against the wind, and make their escape around this edge.

A particular examination of the particles in the stratum of air adjacent to the surface, especially those near the forward edge, showed that they moved at a great variety of angles, nearly normal to the surface at the front and parallel with it at the rear.

The air passing immediately around the edges whirled in eddies on the rear surface, and the main current continued for a short time to travel in lines parallel to the surface of the plane, this movement being a counterpart of that in advance of the plane. These two counter movements formed the first of a series of waves, which continued, sometimes 200 yds. distant, large at first, but gradually decreasing in size, and then disappearing entirely. The waving air, filled with particles of down, presented a peculiar spectacle, suggestive of the squirming of an immense serpent or the waving of a very long flag.

The second set of experiments was performed thus: small metallic planes—the largest 4 in. square—were placed in a draft regulated at will, and a beam of light, from a heliostat, used to light up the floating dust particles, which in the darkness revealed their slightest movements. As these approached the plane the changes already mentioned were noticed, though more marked in some respects.

For instance, when the plane varied about 5° from the direction of the current, some of the particles, having passed the front edge and reaching the surface about one-quarter the distance between the front and rear edges, would turn and, slowly advancing toward the front edge, pass around it, their velocity increasing from the time they turned till they escaped.

In the third set of experiments metallic plane surfaces were placed at various angles in currents of water, and the movements were observed by means of dust scattered on the surface, and of a fiber of silk attached to a slender wire and used as a flag.

The dust revealed the general motions, while the flag served the purpose of examining more carefully the direction of motion in any particular portion. The same phenomena shown by air currents were noticed in these experiments, the great difference in the elasticity of the two media having no appreciable effect in the general results.

In the experiments with water, the pressures at different points were manifested by elevations of the surface. The

water in contact with the front surface of the plane was elevated, and that in the rear depressed. An examination of the elevation on the surface of the plane showed that the point of maximum height moved from the center toward the forward edge as the plane was placed more obliquely to the current.

A study of these changes seemed to indicate—

First, that the location of maximum height was coincident with or not far removed from the location of maximum normal pressure, and, second, that the element of pressure, perpendicular to the direction of the current, is greatest at the front edge, decreasing rapidly from this point in passing to the rear edge.

It is probable that the differences of level on the surface of water find their parallel in compressions and rarefactions of air, in striking a plane. My attempts to investigate this point were unsuccessful for want of adequate apparatus.

In hopes of experimentally determining the general resultant of the forces and movements of fluid particles when meeting a plane obliquely, and hence the surface best suited for receiving them, I studied the effect of planes with a re-entrant angle in streams of water. These were formed of a short and a long plane fastened at a right angle thus ┌──. In this arrangement the point of the short side represented the front edge of an imaginary plane in the line of the hypothenuse.

The water meeting this contrivance traveled in a perceptible curve between the front and rear edges, its movements being made visible by floating scum. These experiments, though instructive, contained elements of error which rendered some of the results doubtful.

In some further investigations light fabrics were fastened by one edge to a wire stretched horizontally, the opposite edge being sometimes loaded to give weight and stability. When placed in a wind, these were thrown into curved surfaces, the rear edge sometimes rising higher than the front; the highest point of curvature being nearly always higher than the front and in advance of the center of the surface.

While these effects were a demonstration of the change of direction given to a fluid current by the disturbing influences of an inclined plane, they were of little value in giving the desired information. This, however, was afterward obtained by an examination of birds' wings, and a study of the actions and reactions of fluid particles, meeting planes under various conditions.

In the examination of the wings of hawks, buzzards, eagles, sea gulls, pelicans, wild geese, and other birds, I found the under surface of the wing from the front to the rear edge a true parabola, varying in its curvature, both according to the relation between the weight of the bird and its wing surface, and the proportion of length and breadth of the wing. A comparison between these various models suggests that the curvature is also affected by the tilt given the wings from the base to the tip.

The parabolic curvature a few inches from the base of the

wing seems most perfect. At this point the front edge is the vertex of the parabola.

In comparing the distance between the front and rear edges —the chord—with the focal length of the parabolic curve, the following order seems to exist. The length of the chord is to the focal length as the weight of the bird is to the wing surface; this relation being affected by the proportion between the length and breadth and the tilt of the wings. I do not give this as an exact proportion, but simply as an indication of the relations. In the pelican's wing the chord is five or six times the focal length of the curve; in the gull's it is three or four times, while in hawks and buzzards it is two or three times.

The difference between the focal length and the chord decreases in passing from the base to the tip of the wing—*i.e.*, the curves become more open; and from the center to the tip the front edge gradually varies from the principal vertex of the (parabolic) curve.

Finally, the curves of the various sections seem to run in constantly diverging lines, the rear portions inclining toward the tip of the wing. As a result of these several variations, the curvature between the tip, rear edge, and the base, front edge, is much greater than that between the tip, front edge, and the base, rear edge.

In some experiments with dried wings these special phenomena were noticed.

If a wing was placed suddenly in a wind so that the front and rear edges were in a line with the direction of the wind, there was no sensible pressure on the under surface; but if the wing was first placed obliquely to the wind, the under surface facing it and slowly turned till the edges were in the position first mentioned, the air continued for some minutes to press on the under surface. Also when this position was reached, the down on the under surface, from the front edge back to nearly one-eighth the width of the wing, continued to be ruffled, indicating a movement of air from this point toward the front edge.

The first two phenomena suggest that when a plane is set obliquely to a current, a structure is given to the fluid movements, which continues to exist after the edges have been placed in a line with the current. This may be the explanation of the continual tipping of birds' wings in soaring.

From these experiments and observations it appears, first, that a plane deflecting a fluid current produces a wave, it being on the crest, the particles approaching it ascending to the surface, and those leaving descending from it; second, that the surface best suited for receiving and utilizing the movements and forces is one having a gradually increasing curvature from the rear to the front edge; and, third, that the curvature of this is dependent on the relation of weight to the surface and the length of the surface to its breadth.

THE MECHANICS OF FLIGHT AND "ASPIRATION."

By A. M. Wellington, M. Am. Soc. C. E.*

In order that the phenomenon in the flight of birds, which, for lack of a better name, has been termed "aspiration," or rising against the wind by power taken from the wind, may be explained, it is presumably desirable to have a correct theory of the mechanics of flight. The theory which I have formed as to the latter differs in some important respects from any which to my knowledge has been suggested heretofore. Otherwise its several elements would seem to me so simple and so evidently true that they might almost be accepted as axiomatic. I shall state this general theory first, therefore, without attempting to establish it in detail, further than to say that, so far as I have been able to observe or ascertain, it is consistent with every recorded fact in regard to the flight of birds and inconsistent with none. In some slight degree the theory of the phenomenon of soaring and aspiration here given is in fact dependent upon this general theory of flight, and therefore I deem it best to state it; but whether the latter be accepted or not, I hope to show that there is an easy explanation of the ability of the bird to rise against the wind through a considerable height, and to continue doing so indefinitely, especially by taking a spiral or zigzag course, and that the supposed mystery of that process, which has been ascribed to some peculiar power or shape of the wings, is really not such, but one much more easily explained and comprehensible.

By "bird" in this paper is meant only that large majority of birds which can maintain flight almost indefinitely if they continue in motion relatively to the air, to the exclusion of birds like the domestic hen which have imperfect or rudimentary flying powers, and of birds like the humming-bird, which have this power so highly developed that, like most insects, they can remain poised in still air at a single point by rapid motion of the wings. It is natural that this latter class, which is confined to quite small creatures, should need and should have much larger wings relatively to their weight than others, and perhaps too much has been claimed for the alleged law that wing ratios grow smaller as the bird grows larger, which is otherwise a most hopeful fact.

For all hopeful studies of the problem of aviation the following propositions should, in my judgment, be accepted as axiomatic. I state them as concisely as possible first, and add some explanatory remarks upon them afterward:

1. At no time and in no degree does any bird use its wings in normal flight as a means of propulsion. Their sole function in normal flight is to create an upward force of buoyancy or "negative gravity," which is approximately equal and op-

* For discussion of this paper, see Appendix A, p. 397.

posite to gravity. According to the will of the bird this "buoyancy" (as it is hereafter called, for the sake of conciseness) is made a little more than gravity when the bird rises, or a little less when it descends.

2. This buoyant force is obtained in two ways, either by (1) flapping the wings in air which is still relatively to the bird (a temporary reliance requiring excessive muscular effort) or through which the bird is moving rapidly (requiring much less effort), or (2) exposing the wings as aeroplanes to air which is moving relatively to the bird, or (3) in part by both. By "soaring" is hereinafter understood such support by still wings moving through air, whether the bird be actually rising or falling at the time.

3. The propelling force of the bird is at all times either (1) a process of sliding downhill upon the air, when gravity is for the time being in excess, precisely as a sled slides downhill on ice, nearly horizontal motion being given by a vertical force, or (2) what is a precise mechanical equivalent, though reversed in direction, sliding uphill on the air, so to speak, when buoyancy is made to exceed gravity, either by flapping of the wings or by taking lifting force from the air in a way we shall shortly see. In many cases, as when a bird is flying with lazy flappings of the wings, it advances by a quick alternation of these modes of propulsion, sliding uphill as it flaps its wings, sliding downhill thereafter until the next flap. In other cases, when merely soaring and circling, it is propelled by the same alternation as will be shown, sliding downhill with the wind and uphill against it, by natural action of physical laws.

4. The bird maintains its stability transversely in two ways, both easily imitated. First and chiefly, in the same manner as a ship is kept upright by having the center of buoyancy considerably above the center of gravity. As a rule, the wings apply their support at about the level of the top of the back, and this alone seems to suffice for transverse stability in many birds; but in most birds it is supplemented by an upward inclination of the wings in soaring, giving them the form of a very flat V, so that any tendency to tip sidewise is checked by an increase of the supporting area on the descending side and a decrease on the other side. By these effective and easily imitated methods the transverse stability of the bird is maintained or increased without conscious effort.

5. The longitudinal stability of the bird is maintained by an action of forces strictly analogous to those which maintain the stability of the old-fashioned steelyard, the tail having, by insensible modifications of its angle to the body, the same function as the moving weight of the steelyard to maintain stability. In each there are only two main forces acting, nearly equal and opposite to each other—viz., gravity downward and the buoyancy or sustaining reaction upward. In both, however, a small adjustable third force is necessary to maintain stability in a horizontal plane, which in the steelyard is obtained by the small weight which is moved in or out, and in the bird is obtained by giving a slight upward or downward

inclination to the tail, against which the air reacts so as to give either an upward or downward balancing force as may be required. In normal soaring flight, motions of the wings, head, legs and body of the bird are neither needed nor used for maintaining stability, though they may be occasionally resorted to

6. It is certain that in the living bird this slight motion of the tail for balancing is effected by an unconscious reflex action of the nerves and muscles, just as the (probably) much more complex action of man's nerves and muscles for balancing the body in walking takes place by reflex action without his taking thought. It is certain that if man's muscular motions for walking depended on his taking thought before each motion, he could walk only very clumsily and slowly, if at all; the process of thinking is too slow. It is equally certain that the living bird cannot take thought for each balancing motion for like reasons. Much more is it certain, therefore, that all attempts at artificial aviation which leave it to man's brain to balance the aeroplane longitudinally by any mode of steering will be far too slow, and are foredoomed to failure. Yet it is not known that this fundamental and crucial necessity for a successful flying machine has ever before been recognized. It is easily secured, it may be noted in passing, by electrical connections with a drop of mercury in a tube which may be set for any angle desired from time to time, since the balancing force, though it must be instantaneous and automatic in its action, may also be made extremely small. Thus we will suppose the probable case that an otherwise successful aeroplane is liable at times to get a sharp pitch downward. It is not necessary for safety that this shall be instantly corrected. It is enough if the body plane (as hereinafter defined) instantly becomes concave on top so as to cause the aeroplane to move in the arc of a circle struck from a center above itself.

7. The sustention of the bird in the air when soaring depends primarily on three facts: (1) The air has mass and is in immediate contact with the bird, so that it is impossible for the bird to fall at all until and except as it causes the air immediately below it to move with it; (2) it is impossible for the bird to cause this air to move downward any faster than gravity alone would cause it to fall in vacuo or even quite so fast; and (3) the velocity with which a body begins to fall is almost inconceivably slow. Though a body falls 16 ft. in one second, it falls only 0.16 ft. in 0.1 second, and only .0016 ft. in 0.01 second, or with a velocity of 0.16 ft. (2 in.) per second. At 50 ft. per second (about 34 miles per hour) the bird passes over 6 in. of solid undisturbed air in every 0.01 second. From the inertia of this air, which resists any sudden change in its line of motion, the bird derives the upward reaction which overcomes gravity. From this it follows that the principal sustaining force must come from the front edge of the aeroplane and not from the back part resting on disturbed air. From this, in turn, it follows that the ideal aeroplane should have a stiff and long front edge, and very little depth longitudinally.

Thus invariably we find wings in nature. Most of the attempts at artificial aviation have done directly the contrary, apparently in the vain hope of securing by length alone longitudinal stability. On the contrary, they tend to make it impossible by prolonging too much the quick process of automatic recovery which is otherwise possible.

8. To derive any sustention from the back part of the wings, which rest only on disturbed air, the back of the wing should dip downward so as to be concave below, so as to follow up and get a reaction from the descending air. That this reaction may be uniform at varying speeds, the rear part should be flexible and springy, so as nearly to straighten out at very high speeds, and by diminishing the angle avoid an excessive reaction. Thus invariably we find wings in nature; thus they have rarely been in attempts at aviation.

9. The concavity of the wings has another useful effect for maintaining stability. The buoyant force is applied a little in front of the center of gravity, thus tending to lift the head up and tail down. By the upward reaction against the back of the wings an opposite rotating force is created, tending to throw the tail up and head down. Thus, stability is almost assured by this concavity of the wings independently of the tail, though it is not apparent why this need be imitated in artificial aviation. The wing planes should be concave to make the air support uniform, but the tail plane alone may be relied on for maintaining stability.

10. Buoyancy in soaring appears to be obtained by having the plane of the wings at a very slight angle with the "body-plane," as we may term it, which latter may be considered to consist of the tail and of a plane equivalent in its effect on the direction of motion to the body of the bird. In a broad sense the wing planes may be included as part of the body plane, it being understood that they are always at a slight angle therewith in normal flight. Though the body is of irregular outline, it is evident that there must always be a plane (or at least a surface which is approximately plane) which would give the same reactions as to direction of motion, and so tend to move in the same direction as the body does in moving through the air. The bird modifies the form, and hence the direction of motion, of this body plane by reflex movements of the tail or wings or both, and, so far as observation, reason and experiment can indicate, keeps the wing plane always at a slightly rising angle from the body plane. The bird assuredly has and exercises the power to vary this slight angle at will, as notably in stopping, when it largely increases it. Whether the bird varies this angle in soaring is much more doubtful, but probably it does somewhat. It may or may not be desirable to preserve the same power in a flying machine. Probably it is not desirable, and at most it can hardly be essential.

11. The bird's loss by atmospheric friction in moving through the air is probably so small as to be almost negligible, measured in feet of fall per mile necessary to maintain a given speed indefinitely. Its form and outer coating has doubtless

been developed to give the least possible air resistance, whether from skin friction or atmospheric displacement, and can only be imitated, as it is most unlikely to be improved upon. The air resistance only of a railway train, which is very ill shaped for this end, being, in my judgment, about $\frac{V^2}{500}$ only in pounds per ton for velocities in miles per hour, neglecting effects of oscillation and concussion, we should, in my judgment, be fairly estimating the total resistance to the motion of a bird at one-tenth as much, which for a velocity of 50 miles per hour gives only 1.28 ft. per mile as the "grade of repose" or rate of fall necessary to balance this resistance. Doubling or quadrupling this estimate, it is still a small matter relatively to the forces to be now considered, and we neglect it, not as non-existent, but as not materially modifying if included, while simplifying by its omission, the computations which follow.

12. (Reaching now more directly the phenomenon of aspiration, or of taking power from the wind and rising or flying in the face of it.) A body moving through space in any direction has in it a certain fixed amount of energy for a given velocity, which is such as would suffice to lift the body through the height through which it would have to fall (in vacuo, strictly speaking) to acquire that velocity. This height varies as the square of the velocity, being four times as great for twice the velocity, and is given in the following little table for each mile per hour up to 60, and thence for each 10 miles to 100. From this table it will be seen that at what we may call the unit velocity of 20 miles per hour, this vertical height (which may conveniently be termed the "velocity head") is 13.38 ft.; for 40 miles 53.51 ft., and so on.

It must be understood that the direction of motion does not matter in using this table, nor how that motion was obtained. Any body which is actually moving through space in any direction with a given velocity, and is in any way guided so as to do so, will of itself and without the aid of exterior force, rise against gravity for the height given in the table; and this vertical height will be the same (neglecting friction), whether the body takes a short vertical or a long oblique path for rising.

And finally, it must be premised that the phenomenon of continuous soaring, or motion for hours together without flapping the wings and frequent rising against the wind, never occurs except when a wind is blowing, and generally a considerable wind of 15 to 20 miles per hour. The best soaring birds, as, for example, the carrion crow, are comparatively poor flyers in still air, and are rarely seen in the air for any length of time except when a wind is blowing; but as the upper air is practically always in motion, this limits their flight only when they wish to remain near to the earth with an eye out for food. The multitudes of birds which do this in all tropical towns begin flying only with the wind and stop flying with it, giving every indication, so long as the wind

Height in vertical feet of lift through which the energy due to any given velocity of motion through space will lift the body against gravity before it comes to rest. (Being the height (h) in vertical feet through which a body must fall to acquire any given velocity (V) in miles per hour. Formula, $h = 0.033445\ V^2$.

Miles per Hour.	0	1	2	3	4	5	6	7	8	9
0..........	0.0	0.03	0.13	0.30	0.54	0.84	1.20	1.64	2.14	2.71
10..........	3.34	4.05	4.82	5.65	6.55	7.52	8.56	9.67	10.84	12.07
20..........	13.38	14.75	16.19	17.69	19.26	20.90	22.61	24.38	26.22	28.13
30..........	30.10	32.14	34.25	36.42	38.66	40.97	43.34	45.78	48.29	50.87
40..........	53.51	56.22	58.99	61.84	64.75	67.72	70.77	73.88	77.05	80.30
50..........	83.61	86.99	90.43	93.64	97.52	101.17	104.88	108.66	112.50	116.42
60..........	120.40	.....	.....	.....	.....	.....	.....	.....	.....	.....
70..........	163.88	.....	.....	.....	.....	.....	.....	.....	.....	.....
80..........	214.05	.....	.....	.....	.....	.....	.....	.....	.....	.....
90..........	270.90	.....	.....	.....	.....	.....	.....	.....	.....	.....
100..........	334.45	.....	.....	.....	.....	.....	.....	.....	.....	.....

blows, that they find resting on their wings in the air rather easier than standing on their feet on solid ground. Whoever has not observed with care the air of careless, lazy ease with which these birds maintain their flight, has missed, perhaps, the best lesson in the art of aviation, and may find it less easy to follow the subjoined explanation of their motions.

We will suppose the wind to be blowing steadily at 20 miles per hour, and the bird to be, for the instant, moving with it, also at 20 miles per hour. Every pound of air has then stored in it 13.38 foot-pounds of energy—a practically unlimited store—and every pound of bird has stored in it the same energy.

Under these conditions the wings have no sustaining power except to delay descent like a parachute. If the bird were suddenly frozen stiff in that attitude and with no relative velocity, it would fall at once to the ground.

To keep on flying when in this condition (which can only obtain for the instant) the bird has choice of two main alternatives, and only two. It may turn against the wind, lose velocity and rise, or it may turn into or with the wind, gain velocity and fall. To continue on wing support it must do one or the other so as to produce a current of air against its wings. It may compromise between the two by taking a more or less oblique course, but one or the other must preponderate.

We will suppose the bird chooses the latter course—to go with the wind. Shaping its path to any slope it likes, it has only to descend through a vertical height of 13.38 ft. or thereabouts (as will shortly be established beyond dispute) to be moving through the air with a velocity of 20 miles per hour, and to be moving through space with a velocity of 40 miles per hour. The potential lift corresponding to this latter velocity is by table 53 51 ft. before the body comes to rest, and $53.51 - 13.38 = 40.13$ ft., as the bird's potential lift before its velocity of motion again falls to the original velocity of 20 miles per hour. The bird, therefore, has only to make a quick (or slow) turn back again into the wind's eye, and before it again returns to its original speed it will rise (on any path chosen by itself) through 40.13 vertical feet, or to a point $40.13 - 13.38 = 26.75$ ft. higher than it was in the first place. It has only to repeat this manœuvre to rise as high as it likes, so long as the wind holds; and it is obvious that circling is merely a more natural and graceful way of performing this manœuvre, either being merely an alternate going with and into the wind.

We will now close the apparent gap in this argument, as above noted. It may properly be asked: If the bird starts with only 13.38 ft. of energy in it, and falls only 13.38 ft. more, or 26.76 ft. in all, how can it possibly have stored in its body at the end of this operation 53.51 vertical feet of energy or any other number of feet in excess of 26.76 ft.?

The answer is: It takes this energy from the wind in falling through it and with it, and in a perfectly natural, simple and easily explained way, which only needs explanation because of our utter lack of practical acquaintance with the processes

of flight. Because of this unfamiliarity, recourse must be had to more familiar conditions to make the process entirely clear.

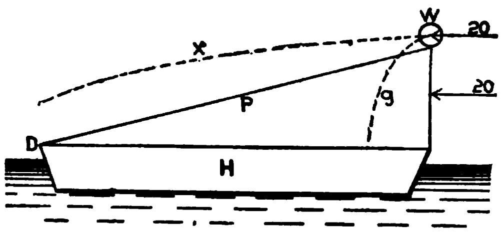


Fig. 1.

In fig. 1 let H be a floating hulk having erected upon it an inclined plane, P, say 13.38 ft. high, as heretofore, being the vertical head due to a speed of 20 miles per hour. Let this hulk and plane be moving horizontally at 20 miles per hour. Immediately above the top of the plane, barely touching but not fixed to it, let there be a spherical mass, W, also at the instant moving horizontally through space at 20 miles per hour, and so for the instant at rest relatively to the top of the plane, but having its gravity resisted by it.

Now, if the hulk were fixed and stationary and the mass W also at rest where shown, there can be no doubt what would take place. The mass W would begin to roll down the plane, and at the point D at its foot would have acquired the velocity due to the fall of 13.38 ft. = 20 miles per hour, and would be rolling ahead at that rate.

Similarly, under the conditions assumed, there can be no doubt what would happen to the mass W except for the hulk and plane. It would begin falling in a parabolic path, g, like any other projectile, according to well-understood laws, and would strike the ground (or deck) with the velocity due to its original energy plus 13.38 ft. more of fall, being that due to 13.38 + 13.38 ft., being, by the table given, 28.28 miles per hour.

But under ALL the conditions assumed, what will take place depends upon a further fact: the relative mass of W and H-P. As to that we will assume two extremes: First, that the mass W is very great, and that the mass H-P, though having great buoyant power, is made of some as yet unknown material, and is very small in weight; and secondly, that the reverse conditions obtain, the mass W being very small and the mass H-P very great.

Under the first assumption it will be obvious that the great mass W will pursue its natural downward path g regardless of the trifling mass H-P, whose motions it will control to suit itself. In doing so it will absorb whatever energy there is in H-P, being thereby pushed out into a slightly longer parabolic

path than its natural path g, and striking the ground with slightly greater force, having in the meantime brought H-P to rest; but sensibly its path will be unaffected. On the other assumption, that W has relatively trifling mass, it will still try to take the dotted path g, but the plane P will not permit it to do so. Instead of that, the plane P will wholly control W, and will keep pushing it ahead, pushing it ahead, as it descends, compelling it to take through space some much prolonged path x, and all the time imparting successive installments of moving energy to it in order to compel it to take this path, until, when W has descended to the point D, it will be actually moving through space with the velocity of (nearly) 40 miles per hour, because it will be moving relatively to the plane at 20 miles per hour. To acquire this additional energy represented by the higher velocity, it has, of course, had to take it from H P, and by so much has decreased the velocity of H-P; but if the mass of the latter be very great relatively to W it will not have been sensibly decreased in velocity despite the large increase it has given to W.

In this latter instance we have a precise parallel to what takes place when the soaring bird is sailing with the wind, and this once clearly seen, all mystery disappears from the process of aspiration, and everything that soaring birds are seen to do in air becomes easily explicable. I have not undertaken to write a long paper, and therefore cannot now follow through these manœuvres in detail; but it will be obvious that the mass and moving energy of the wind are practically infinite relatively to the bird's mass. The bird cannot diminish it by taking from it, and lateral friction keeps the mass of air in immediate contact with the bird moving synchronously with the adjacent air regardless of the slight tendency to hold it back due to the bird's inertia.

All the recorded instances of circling and rising without work, such as that given in page 79 of Mr. O. Chanute's forthcoming work on "Flying Machines" (repeated in fig. 2 herewith), are thus easily and completely explained. So also is the incident narrated on page 77, from Mr. Chanute's own experience, where a gull spread his wings on top of a post in a wind of 14.4 miles (= 7 vertical feet of energy) and first "rose vertically 2¼ ft." (equivalent work by the wind to giving the gull a velocity of 9 miles per hour), "then drifted back about 5 ft., still rising slightly" (equivalent to communicated work of, say, 10 miles per hour), "then advanced against the wind, losing a little height" (giving an impact velocity of, say, 16 miles against the wind), "and was thenceforth in full soaring velocity." Which means that the bird first permitted itself to rise and drift back in order to gain height, and then gave a quick turn and went with the wind to store energy in its body, for it could not possibly have risen into the wind's eye at the stated instant without thus first charging itself with energy, unless by some exceptional action like the following:

On the same page there is narrated a case where an eagle is said to have "first descended 7 to 10 ft., and then risen direct-

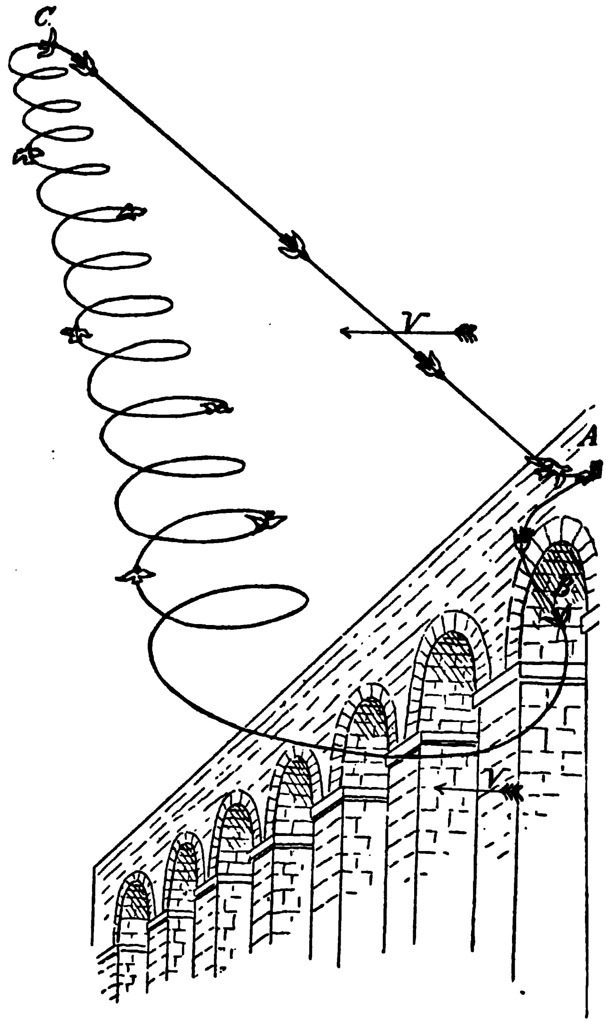


Fig. 2.

ly and slowly some 300 ft. into the air while advancing against the wind some 150 ft. into the air."

If this incident really occurred precisely as stated, as it easily may have done, it required a balance of forces analogous to fig. 3, which is more difficult to explain with certainty, and which we may be sure is beyond imitation by man until the art of artificial flight has made great advances, since it must depend largely on quick reflex action of all the muscles, which circling does not. If the bird set its body plane pitching forward at a (relatively) sharp angle, so that gravity (G, fig. 3)

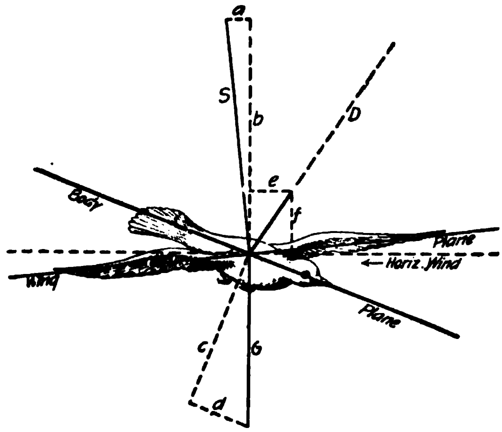


Fig. 3.

tends to make it slide forward and downward, and then sets its wing planes at a somewhat greater angle than usual to the body plane, so that the reaction of the wind creates a buoyant force, S, greater than gravity, which is nearly all "lift" and very little backward "drift," we have the primary forces S and G, which may be resolved into opposing vertical and horizontal forces, as in fig. 3. Canceling these opposing forces, so far as they are equal and opposite, we have left an unbalanced upward and forward resultant which will naturally tend to make the bird rise very slowly in the general direction D, as described. As shown, fig. 3 requires the introduction of some other small force to give the required equilibrium, which may be derived in many ways, but can only be guessed at.

I have never observed just this manœuvre, recognizing it as such, but I have often observed what I believe to be a still more difficult phenomenon, that, namely, of "hovering" almost motionless for a sensible period over a particular point. This can be done in a wind: only for a short time, and only when the bird is headed directly into the wind; but it is

certainly often done. While doing it, however, the birds have all the air of a tight-rope performer, quivering and balancing with much apparent effort, in marked contrast to the non-chalant ease with which they rise in circling. Both of these phenomena I believe to be mere tricks of balancing, forever beyond imitation by man, while ordinary circling and soaring are not so by any means, for any visible reason.

It is quite possible, however, that the record of eagle flight above given was erroneous because of optical illusion due to the fact that the eye observes the motions of birds easily only in a plane normal to the line of sight. All motions in other planes are unconsciously observed as if projected on to that plane, the components of motion in a line to or from the eye being lost. What the eagle really did may have been to zig-zag upward by pointing his head first into and then away from the wind. It is obvious that the alternations of velocity which we have found to be a necessity for soaring may be obtained as easily be zigzagging as by circling. The alternations will not be so great, nor hence the lifting force, but they may be obtained more rapidly and with less loss of distance.

For example, a flight in reversed arcs of 90° is only 11.1 per cent longer than straight flight along the chord lines. If the latter be at an angle of 45° with (and against) a wind of 20 miles an hour and the bird maintains a steady motion of 20 miles per hour through the air, its velocity in space will be alternately 0 and 28.28 miles per hour, as brief computation will show, giving abundant lifting force which the bird can use either to rise higher or to increase its velocity, as it will. At high flying speeds the arcs may be much flatter and still gain lifting force.

Only by the methods here given can the work done by good flying birds be explained. If the muscular effort were as great as indicated by many of the computations which have been put forward, a few hours of flight would burn any bird up completely, even if it obtains its power at the theoretically perfect rate of 0.18 lb. of carbon per H.P. hour.

If the conclusions so far reached in this paper be accepted, it is obvious that they greatly simplify the problem of artificial flight by reducing to a minimum the demand for power, making it chiefly necessary for acquiring the first initial velocity. All attempts at aviation which include any motor for propulsion are, in my judgment, on wrong lines and predestined to certain failure, since they not only neglect but destroy the action of the forces by which true flight may be and is attained. I will not go so far as to say that some birds, in the exuberance of power, may not use the wings to accelerate as they do to retard motion. I think they do, but only in an abnormal way ; it is wholly unnecessary and even destructive of all normal flight. The fish needs a propeller because it has no gravity in water ; the bird does not need it because he has gravity, and in that gravity has the best and smoothest of all conceivable means of propulsion if he can make the wind lift him uphill whenever he has slid far enough downhill.

If so, man commits an absurdity when he flies in the face of nature and assumes a propelling force where none is needed or exists. Some other errors which, in my judgment, have foredoomed to failure all efforts at aviation so far made are the following:

1. Much disappointment has arisen from seeking a screw which would lift itself in still air. No experiments have yet been made, to my knowledge, to determine the vertical lift of a screw while it is at the same time being moved rapidly through the air horizontally. By mounting such a screw on the end of a long revolving arm, a surprising difference in vertical lift would be observed, I believe. If so, we may easily arrange to give any flying machine a horizontal motion of 20 to 40 miles per hour relatively to the air before lifting effort begins, and as lifting effort may stop as soon as a moderate velocity and any moving current of air is reached, the probable demand for power is greatly reduced below the present impressions.

2. The function of the tail as an automatic vertical rudder, and the absolute necessity of imitating the quickness of reflex animal motions by automatic mechanism, has been never before noted to my knowledge, and at least has been generally neglected.

3. So obvious a necessity as that the center of buoyancy should be at some considerable height above the center of gravity has been neglected in most of the prior attempts at aviation, as illustrated in Mr. Chanute's forthcoming treatise on the subject; yet every flying creature without exception may be observed to have this peculiarity.

4. The teachings of reason and nature in favor of making the supporting aeroplanes in the form of a flat V instead of a plane have usually been neglected. As the bird has the power of partly withdrawing the wing by reflex action, it has less need of this form than man, yet nearly all birds carry their wings so in soaring.

5. There has been an inexplicable tendency to make aeroplanes long and narrow instead of wide and shallow after the analogies of nature. This, in my judgment, is a particularly grave error, because all or nearly all of the support comes from the front edge of the aeroplanes resting on undisturbed air. All flying creatures have wings conforming to this theory.

The greatest obstacle to making any real progress toward the art of aviation lies in the extreme difficulty and danger of making the first experiments. That difficulty can never be removed wholly, and until it has been sufficiently, clumsy errors of detail are inevitable in first attempts; but apart from that the problem in abstract mechanics, in my judgment, reduces itself to this: How to give an upward movement slightly in excess of gravity to an aeroplane imitating pretty closely the bird's form and weight per square foot of plane, and already having horizontal velocity, until a considerable height and tolerably steady current of moving air can be struck. That done, there seems to be no mechanical obstacle (other

than lack of practice) why further lift and horizontal progress in any direction should not be continued indefinitely without further use of power, and why safe alighting without the use of power should not also be possible.

As the problem has interested me only in an abstract way, I leave to others the problem of putting these conclusions to test; but that the problem is forever beyond man's power to solve I see no reason to believe.

ON THE ACTION OF A BIRD'S WING.

By B. Baden Powell.

It seems extraordinary that, after all the careful observations of scientists during many centuries, we cannot at the present moment say that we thoroughly understand the exact action of a bird's wing during flight. Much has been done in recent years, however, and the general principles have been mastered, though many details remain undetermined.

Great conclusions may be obtained from a number of trivial observations, and it becomes the duty of every one to add what little he can to assist in the great problem. I therefore propose, without taking up much time by detailing all the experiments I have made and the conclusions I have come to, to merely describe one action of a bird's wing which I have not seen referred to in any of the numerous articles I have read on the subject.

M. Marey and others have described the more or less circular trajectory of the bird's wing; but this only refers to the humerus or lower joints, and but little has been said about the outer joints, the flexing and stretching of the wing.

If an observer is able to get directly underneath a flying bird —though such a point of view is not as a rule easy to attain— he will observe that the bird simply appears to alternately extend and flex his wings. They are spread out wide at one instant and contracted inward at the next.

It is therefore probable that the wing during the down stroke is well spread out to take advantage of the greatest area for compressing the air beneath, while during the up stroke the wing is flexed.

In order to prove this (as well as other movements of the wing) I set up a simple piece of apparatus which proved a beautiful and most successful method of demonstrating what I wanted to prove. I constructed a cylinder of paper of a diameter such as to about equal the spread of a sparrow's (or other bird's) wings. The interior of this tube was smoked, and a bird being put in at one end, flew through the tube, the tips of his wings scratching off the smoke as he flew. When the cylinder is cut longitudinally in two a very clear diagram is obtained. An even simpler plan for showing certain actions is to hang two pieces of blackened paper face to face at a dis-

tance apart equal to the spread of wing, and then let the bird fly between the sheets.

These experiments show most conclusively that the bird's wing must be flexed during the up stroke, as there is scarcely any sign of a scratch by the feathers during the up stroke.

I have before me a specimen—the result of a canary flying between two sheets of blackened paper. Each wing stroke has rubbed out an imprint somewhat resembling the whole outline of an interrogation point; the fully extended wing, at the top of the stroke, first rubbing out a broad curved streak, and then gradually tapering this streak upon a curved line to a point as the wing became flexed upon the down stroke and only rubbed by its extreme tip.

The experiment is very simple, and can easily be repeated by any one.

I shall hope at a future date to be able to make some more experiments in this line.

NOTES ON THE DESIGN OF FLYING MACHINES.

By J. D. Fullerton, Major Royal Engineers.

Section 1.

DEFINITION OF A FLYING MACHINE.

(1) In these notes, by "flying machine" is meant a machine heavier than air, which contains its own motive power.

Section 2.

COMPONENT PARTS OF A FLYING MACHINE.

(2) Numerous forms of flying machines have been proposed, but they all consist, in some form or other, of the following parts (see fig. 1):

a, Sustainer or aerosurface, which rests on the air.
b, A car for passengers, etc., attached to the sustainer.
c, Propelling machinery.
d, Steering apparatus.
e, Balancing apparatus.

Section 3.

POINTS TO BE CONSIDERED WHEN DESIGNING A MACHINE.

In designing such a machine, the following points must be taken into account:

I. *Aeronautical Considerations.*

A. The motion of a machine through the air, the forces acting upon it, and the conditions of its equilibrium.
B. Rising from the ground.

C. Descent.
D. Change of direction in the air.
E. Testing the equilibrium.
F. Motion of aerosurfaces and bodies through the air, lifting power, resistance to forward motion, position of center of pressure.

II. *Mechanical Considerations.*

A. The construction of sustainers and aerosurfaces.
B. The car and its attachments.
C. Propelling machinery.
D. Steering apparatus.
E. Balancing apparatus.

Aeronautical Considerations.

MOTION OF A MACHINE THROUGH THE AIR—FORCES ACTING.

A. When a machine moves through the air the forces acting upon it are:

(*a*) Air pressure on the aerosurface acting through the center of pressure of the surface This, practically speaking, is the force which sustains the machine in the air.

$A \cos. \alpha = P =$ vertical sustaining force.

$A \sin. \alpha = R_1 =$ horizontal resistance to the motion of the aerosurface.

$R_2 =$ horizontal resistance to motion of the car attachments.

$R_3 =$ horizontal resistance to motion of the car.

$R_4 =$ horizontal resistance to motion of wheels when in motion on the ground.

$L =$ wind force on the aeroplane (case of a directly opposing wind).

$L \cos. \alpha = K =$ vertical component of wind pressure.

$L \sin. \alpha = R_5 =$ horizontal component of wind pressure.

$T =$ thrust which must be provided by propelling machinery.

$W =$ weight of machine.

$R = R_1 + R_2 + R_3 + R_4 + R_5$.

(Some small, unimportant forces can be omitted.)

CONDITIONS OF MOTION AND EQUILIBRIUM.

Then the conditions of motion and equilibrium are:

1. $P + K$ and W must act in the same vertical straight line through the center of pressure of the aerosurface.
2. T must $= R_1 + R_2 + R_3 + R_4 + R_5 = R$ and must act in the same horizontal line with R
3. When $P + K$ is greater than W the machine must rise.
4. When $P + K$ is less than W the machine will fall.
5. With the thrust $T = R$ the machine will proceed horizontally with a certain velocity, when $P + K = W$.
6. As velocity, and consequently T, increases, $P + K$ will exceed W, and the machine will rise.

7. If the velocity diminishes, $P + K$ will be less than W, and the machine will fall.

Fig. 1.

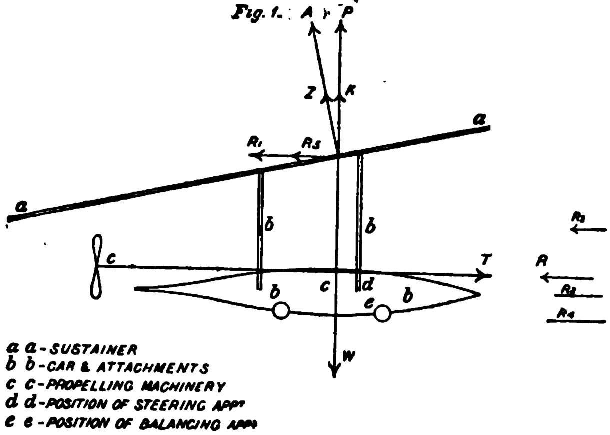


Fig. 5.

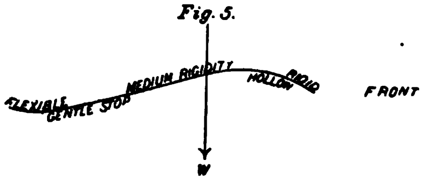


Fig. 6.

Fig. 7.

8. The H.P. required to drive the machine at any given speed is

$$= \frac{R \, . \, v}{550},$$

where v = given velocity in feet per second, R = total horizontal resistance in pounds.

CALCULATION OF POWER REQUIRED.

A convenient method of calculating the thrust, and consequently the total H.P. required for the movement of a machine under given circumstances, is shown in figs. 2, 3 and 4.

Fig. 2 shows the trajectory of the machine under the given conditions; for instance, it might start from *a* and rise in the air at *b* when a certain given velocity had been attained. Continuing with the known increasing velocity, it would reach *d*, then rise again, etc. The time taken and distance run is of course also known.

Fig. 3 shows the horizontal resistances to be overcome, R_1 R_2 R_3, etc.; these are shown below the base line, as is also the resistance of the mass when the velocity is variable.

Fig. 4 shows the vertical forces—viz., the weight, upheld in the air by the vertical components of the air and wind pressure. The small effect of the mass when the rising force is variable may be neglected. In order to ascertain the thrust at any particular moment—say when the machine is at *f*—measure the resistances $g\,h + i\,j + k\,l + o\,p = r\,s$ = total thrust required, and from this the H.P. can be calculated as above explained. The curves for R_1 R_2, etc., can be found, as explained hereafter.

CONDITIONS OF LONGITUDINAL EQUILIBRIUM.

(*b*) As already explained, the condition of longitudinal equilibrium is that $P + K$ and W must act in the same vertical straight line. This is obvious, as if they do not a couple would be formed tending to turn the whole machine over backward or to tip it suddenly forward.

This is a very important point, as it is extremely difficult to insure the conditions above laid down. Passengers must move about the car, stores, ammunition, etc., will be expended, and small variations of the load, with the consequent alteration in its line of action, will constantly take place. There appear to be several remedies for this difficulty—viz.:

METHOD OF ATTAINING EQUILIBRIUM—FLEXIBLE BIRD'S WING AEROSURFACES.

1. Using aerosurfaces similar in form and flexibility to the wings of a bird. The important points to notice about this form of aerosurface are the peculiar shape (see fig. 5)—viz., deeply hollowed front, with gently sloping rear surface, and the flexibility of the rear portion. A bird in full flight is constantly changing the line of action of its weight, and it is only prevented from losing its equilibrium by the flexibility of its wings, which alter the position of the center of pressure of the aerosurface to suit the alterations in the line of action of the weight. It is also assisted very materially by the shape of the wings, which are so formed that the center of pressure coincides, or nearly coincides, with the center of form at all speeds, thus rendering the equilibrium much more stable and less susceptible to alterations in the line of action of the weight.

ADJUSTMENT OF INCLINATION OF SURFACE.

2. By adjusting the angle of inclination of the aerosurface to suit the particular weight. This is the arrangement adopt-

ed by Mr. Maxim, who uses a plane, or nearly flat aerosurface, which he fixes in a suitable position by trial before leaving the ground. It is extremely doubtful whether any adjustment of the angle of inclination could safely be made while in the air ; great power would be required, and very small alterations in the angle of inclination would considerably alter the lifting power.

MOVABLE WEIGHTS FOR CORRECTING BALANCE.

3. By using a movable weight which the steersman would work backward and forward as required. In the French balloon experiments in 1885 it was found that the balloon and car had a tendency to rise in front as speed was got up. This was, of course, due to the alteration in the conditions of equilibrium, precisely as explained above. To remedy this defect a movable weight was used, and it was found very efficacious ; but it must be recollected that the speed of the balloon did not exceed 15 miles per hour, while that of flying machines such as those now under consideration will probably be from 6C to 100 miles per hour.

GYROSCOPIC BALANCERS.

4. By using heavy flywheels, something on the principle of the gyroscope. It is believed that this is now being tried ; it would probably be possible to revolve the weights by means of fans, something like windmill sails, which would be driven by the air pressure in the same manner as the weight is sustained by it.

If this is done, the resistance of these fans must be allowed for in calculating R as above described.

The above are some of the methods which might be found suitable : probably a combination of all of them would give the most satisfactory results.

(*c*) Crosswise stability can be insured by making the outer parts of the aerosurface flexible (fig. 6), or by using the form of aerosurface shown in fig. 7. The former seems to be the best arrangement. Here again the great advantage of a flexible aerosurface is shown, as the turning up of the tips of the wings as automatic, requiring little or no exertion on the part of the bird.

B. The best method of rising from the ground appears to be by running the machine along a sort of railway track ; then when $P + K$ is greater than W the machine will rise in the air. It is desirable that the rising speed should not exceed about 40 miles per hour, as in the event of the permanent way breaking, the damage to machine and passengers would not be so great as if the speed was 70 or 80 miles an hour.

DESCENT AND SAFETY.

C. Safe descent is perhaps the most difficult part of the problem of flight to solve. The points to be attained are :

1. The descent must be gradual.

2. Both horizontal and vertical velocity must be low when reaching the ground, and the latter should, if possible, = 0.

3. In case of breakage to the machinery the arrangements must be such that a safe descent can be made. As above explained, the machine will descend when $P + K$ is less than W. As long as the thrusting force is in action the vertical velocity can always be kept within reasonable limit, as the greater the horizontal the less the vertical velocity. But a high horizontal velocity is also objectionable, and it is very desirable to descend with as low velocities (horizontal or vertical) as possible.

SAFE PROPORTION OF WEIGHT TO AEROSURFACE.

This can be attained:

1. By making the aerosurface so large that even if it descended vertically downward without any horizontal velocity its speed would not exceed a certain amount. Usually this is considered to be 22 ft. per second, and can be arranged for by making the weight of the whole machine not greater than 1 lb. per square foot of aerosurface (considering the surface as a flat plane). The objection to this is that even in large machines only a small weight can be carried, and all the propelling apparatus, etc., must be specially designed for lightness.

2 By using auxiliary wings or sails which are stowed away while in transit, but open automatically on touching the ground or when required.

This plan is being tried by Mr. Maxim. The chief difficulties lie in stowing away the auxiliary sails, the risk of their failing to act when required, and the alteration of the balance of the machine when they are taken into use.

3. By using springs in the base of the car, to take the shock. This could only be done to a very limited extent.

4 By having specially constructed landing-places which would allow of a machine coming down with a high horizontal or vertical velocity. Probably the best arrangement would be a combination of these plans, but particular attention must be paid to the case of a sudden breakage of the machinery, and consequent loss of thrusting or propelling power.

CHANGE OF DIRECTION—TWIN SCREWS.

D. The best method of changing direction is to use twin propellers, working one faster than the other in the manner usual on board ship. There should, however, be a subsidiary apparatus—a weight which can be moved from side to side of the machine, thus altering the position of its center of gravity and changing the direction of motion. This is the method employed by birds.

TESTING EQUILIBRIUM.

E. In order to insure equilibrium and proper balance it is necessary—

1. That the lines of action of the weight and air pressure must be in the same straight line.

2. That the line of action of the thrust T must be in line with the resistance R.

With models this is comparatively easily arranged for. The position of the center of pressure of the aero-surface can be found by fixing it in the whirling machine (described hereafter), and the adjustment of the weight is a simple matter. The second condition of equilibrium can then be arrived at by putting the complete model into the whirling machine and by trial adjusting the lines of the thrust and resistance as explained.

This is more difficult for full-sized machines. The best plan appears to be to calculate carefully, as far as calculation is possible, and then to make accurate large-sized models which can be tested in the whirling machine, as explained above. Mr. Maxim's method of testing is to run the machine on a pair of rails arranged to touch the upper or lower edges of the wheels. By adjusting his aeroplane he can then make his machine run level on the upper rails, and so insure the equilibrium being correct before leaving the ground.

MOTION OF AEROSURFACES.

F. In order to ascertain the lifting power, resistance, etc., of aerosurfaces it is necessary to use a "whirling machine." This is simply a machine which drives the surface to be tested round in a large circle at a high rate of speed. Attached to the machine are delicate apparatus for measuring the lifting power, resistance, power required to drive the surface, position of center of pressure, etc.

A very complete description of a whirling machine is given in Professor Langley's work entitled "Experiments in Aerodynamics," and in *Engineering*, 1893, drawings are given showing Mr. Phillips's apparatus. The general principle of all such machines is the same; but a considerable amount of ingenuity can be expended in the design of recording and measuring apparatus. It is not necessary to explain the details of the different apparatus, as they are fully explained in the papers above mentioned. A short account of the experiments tried by Professor Langley and Mr. Phillips is, however, desirable, as the numerical results obtained enable very fair approximations to the values of the different forces acting on a machine in flight to be made.

RESISTANCE TO VERTICAL PLANES PASSING THROUGH THE AIR.

If v = velocity in feet per second, S = surface in square feet, then resistance to forward motion $= S \times .00166\, v^2$.

It is not known yet whether this result is correct when S is very large: possibly the resistance is smaller, but probably the formula is sufficiently accurate for the estimation of the forces.

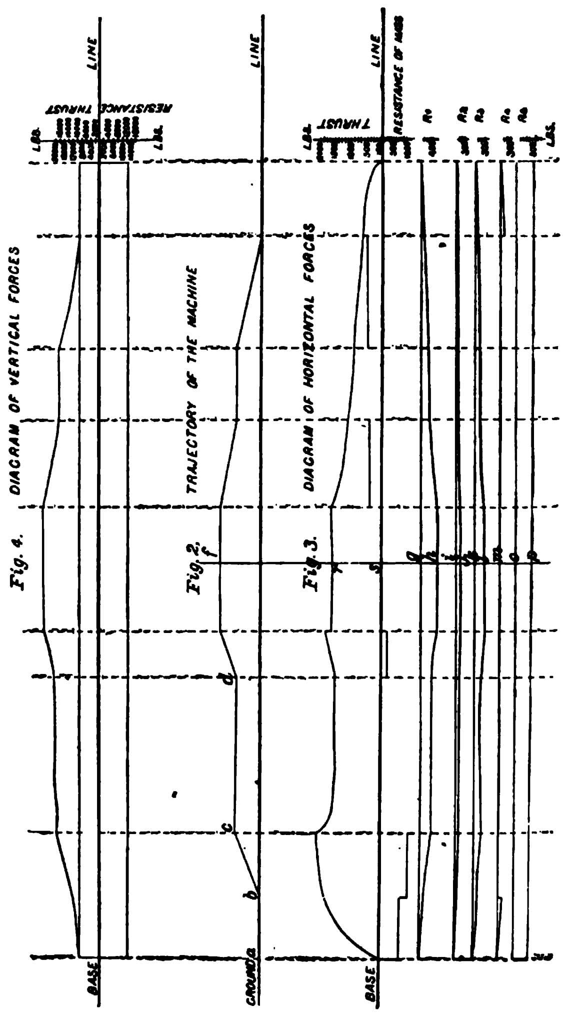


Fig. 4.
DIAGRAM OF VERTICAL FORCES
RESISTANCE
THRUST
LBS.
LINE
BASE
Fig. 2.
TRAJECTORY OF THE MACHINE
LINE
GROUND
a
b
c
d
f
Fig. 3.
DIAGRAM OF HORIZONTAL FORCES
THRUST
RESISTANCE OF MASS
LINE
BASE
LBS.

Wolff's table ("The Windmill as a Prime Mover") gives:

Resistance $= .005\ c^2\ S$, where $c =$ velocity in miles per hour.

This is higher than Langley, and agrees better with Smeaton and Rouse's experiments. Langley's values will, however, be on the safe side.

RESISTANCE TO INCLINED PLANES.

Professor Langley's experiments show that the lifting power of inclined planes or sustainers is far greater than was estimated by Newton. A very convenient table showing the amount of "lift" and "drift" (resistance to horizontal motion) is given in Mr. Chanute's papers on "Progress in Flying Machines," in THE RAILROAD AND ENGINEERING JOURNAL, Vol. 1891, p. 462. From these tables the amount of the forces acting on a machine can be calculated and the diagrams (figs. 3, 4 and 5) drawn for any particular case.

RESISTANCE AND LIFTING POWER FOR OTHER FORMS OF AEROSURFACE.

Experiments on other forms of aerosurface are much required; but there is little difficulty in ascertaining the "lift," "resistance," etc., of small surfaces by fixing them in the whirling table. At present, however, the only satisfactory experiments appear to be those of Mr. Phillips (*Engineering*, 1893), who found that while with his peculiar form of surface the resistance to forward motion remained about the same as for an equal-sized plane surface, the lifting power was three and four times as great. The particular shape advocated by Mr. Phillips is in section like a bird's wing. While experimenting on this subject it is very desirable to ascertain the result of using flexible aerosurfaces, for reasons given above.

RESISTANCE TO MOTION OF BODIES.

No very clear results have as yet been published as to the amount of resistance offered to solids or bodies moving through the air, and information is much required on this point, with a view to determining the best form for the car. Probably for very high speeds the fish shape will be found to be the best. The French aeronauts consider that for balloons the largest diameter should be at 40 per cent. of the length from the front, and not in the center.

The usual calculation made for the resistance of a "fair-shaped" body is from $\frac{1}{10}$ to $\frac{1}{20}$ of the resistance offered by its largest cross-section, the resistance decreasing as the ratio of the length of the body to the largest diameter increases.

POSITION OF THE CENTER OF PRESSURE OF PLANES.

It has been found by Professor Langley and other experimenters that the center of pressure of an inclined plane moves

forward as the angle of inclination decreases—that is to say, the centers of pressure and form do not coincide. This can easily be proved in the whirling machine. Langley's rule is:

$$d = (0.3 - 0.3 \sin. \alpha) L$$

where d = distance between centers of pressure and form.

L = length of plane.

POSITION OF THE CENTER OF PRESSURE OF OTHER SURFACES.

For surfaces of other form no results are as yet published; but it is usually considered that in the case of birds' wings, or aerosurfaces of that description, the center of pressure is at a distance equal to ¼ the length from the front edge. Of course, for small aerosurfaces the exact position can easily be found in the whirling machine. It is not quite clear yet whether the position of the center of pressure alters with the velocity; the probabilities are, however, that the change, if any, is, for high speeds and small angles, not very great.

II. *Mechanical Considerations.*

CONSTRUCTION OF SUSTAINERS—MATERIALS.

A. The construction of aerosurfaces or sustainers is a matter of the greatest importance, as on the efficiency of the aerosurface depends the safety of the whole machine. Its size and dimensions having been settled upon, as above explained, the materials for its construction are the next consideration. These should be for all framework, steel tubing for the more rigid parts, while for the more flexible portions flat steel bar or ribbon will be found useful. For covering, stout cloth interwoven with wire would seem suitable, the whole being rendered fireproof by means of composition, etc. The cloth should be cut to fit accurately, and so arranged that its fiber is in the most advantageous position for resisting the strains likely to be brought upon it.

Particular attention should be paid to testing all materials, and the aerosurface when completed should be examined and its strength tried by weights, etc.

CAR, ETC.

B. These do not call for any very special notice, as the framework employed will be very similar to that used in yacht-building and such like work. Attention, however, may be drawn to the desirability of constructing a thoroughly strong car framework, as this framework really forms the bed for the engines, and it consequently must be well able to withstand the shaking and jolting caused by the motion of the machinery. It is also desirable that the attachments should afford as little resistance to forward motion as possible.

The car should have space for propelling machinery, steering and balancing apparatus, fuel, warlike stores, etc., and

there should be a kind of conning tower with glass front for the steersman. Arrangements should also be made for looking vertically downward through the bottom, and for firing shells, etc., in the case of war machines. The mode of propulsion—viz., screw or jet—will considerably affect the car construction.

PROPELLING MACHINERY.

C. All propelling machinery consists in some form or other of (1) The propeller; (2) the engine for working the propeller; (3) the apparatus which generates the working fluids; and the object of all such machines is to drive the car, engine, or other vehicle along by forcing out at high speed air, gas, water, etc., and thus gaining a reaction from the atmosphere. The amount of this reaction, which is the thrust T, opposed to the horizontal resistance R, is always = the mass driven out per second × the rate of acceleration per second. It will be seen, therefore, that the thrust depends upon the weight of air, gas, etc., put in motion and the velocity with which that air, gas, etc., moves.

PROPELLERS.

At present the most suitable form of propelling machinery appears to be the screw propeller. Information as to the best form of fan is much required; this can be tested very well in the whirling table. Professor Langley and Mr. Maxim have made a number of experiments on this subject, but as yet no trustworthy details have been published. One thing, however, is pretty certain—viz., that a propeller in forward motion cannot be calculated in the same manner as a stationary fan or windmill; but it is probable that if the resistance of the fan to forward motion is taken into account a very fair notion of the power required can be obtained.

ENGINES.

The general design of these for steam or gas is well known. The principal point to be attended to is to make all the parts as light as possible consistent with strength. The ordinary engines are made much too heavy. There should be no difficulty in reducing the total weight of steam engines (exclusive of coal and water) to 10 lbs. per I.H.P. All connecting rods, etc., should be made hollow, and the supporting pieces can be lightened in the same way.

GENERATORS.

The chief weight in all engines at present is in the generator. A great deal can be done to overcome this difficulty by using "instantaneous generators," which only make the exact amount of steam or gas required per second—the regulation of the amount being arranged for by special apparatus. A good example of this is Mr. Maxim's new machine, which is very light and uses a very small amount of fuel.

A condensing engine is absolutely necessary for aerial work. A very good condenser can be made by utilizing the framework of the aerosurface in the manner proposed by Mr. Maxim.

GENERAL REMARKS ON PROPELLING MACHINERY.

At present with either gas or steam it is impossible to get more than about 20 per cent. of the available work of the original substance used to drive the engine, and it is extremely doubtful whether this can be improved upon. The fuel also is not likely to go below 1 lb. per I.H.P. per hour; but the weight of the working fluid can be greatly economized by using the same fluid over and over again. Till lately there were many difficulties in the way of doing this; but Mr. Maxim's application of an aerial condenser seems to settle this question.

It seems worth while to inquire whether some form of jet propulsion could not be used. Jet propulsion for water is a failure because it is impossible to give sufficient velocity to the water; but explosive mixture of gas can be driven out at enormous velocities, and it is possible that the want of weight in the gas might be compensated for by the increased velocity.

It may be here mentioned that for models small rockets are excellent motors. It is true they do not last long, but, on the other hand, they are very powerful, and consequently drive the model at a very high rate of speed. This is very important, as errors of balance, etc., show much better at the higher speeds.

STEERING APPARATUS.

D. As already explained, the best form of steering apparatus is the twin screw propeller. The subsidiary apparatus need only be a weight sliding right and left on a bar, care being taken to see that it is easily controlled by the steersman. The amount of weight can easily be calculated, or for models found experimentally.

BALANCING APPARATUS.

E. The principles of equilibrium have frequently been alluded to, and for rising and moving along in the air, flexible aerosurfaces, moderately weighted, appear to be all that is necessary. But some special apparatus is required to insure safe descent, and it is not easy to decide which is the most suitable. Further experiment on this subject is much required.

SECTION 4.

These notes do not pretend to be an exhaustive treatise on this subject; but it is hoped they may be found useful to those who are studying the problem of flight as a handy form of reference for matters to be considered.

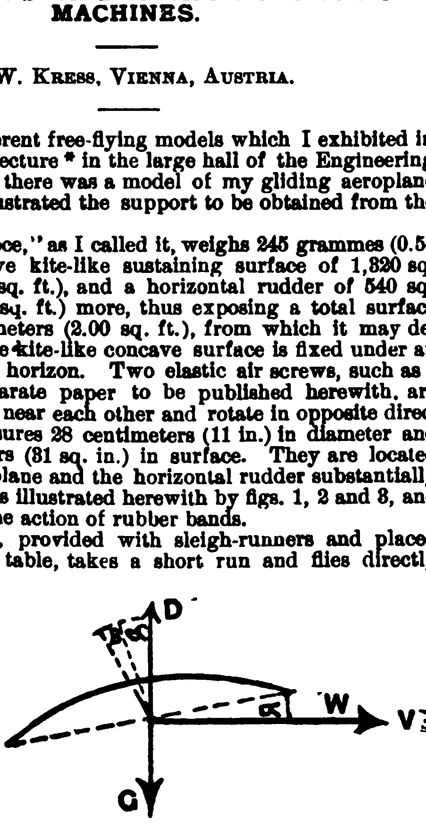
AEROPLANES AND FLAPPING FLYING MACHINES.

By W. Kress, Vienna, Austria.

Among the different free-flying models which I exhibited in my experimental lecture * in the large hall of the Engineering Society in Vienna there was a model of my gliding aeroplane or kite, which illustrated the support to be obtained from the air.

This "Aeroveloce," as I called it, weighs 245 grammes (0.54 lbs.), has a concave kite-like sustaining surface of 1,320 sq. centimeters (1.42 sq. ft.), and a horizontal rudder of 540 sq. centimeters (0.58 sq. ft.) more, thus exposing a total surface of 1,860 sq. centimeters (2.00 sq. ft.), from which it may derive support. The kite-like concave surface is fixed under an angle of 3° to the horizon. Two elastic air screws, such as I describe in a separate paper to be published herewith, are affixed to the kite near each other and rotate in opposite directions. Each measures 28 centimeters (11 in.) in diameter and 200 sq. centimeters (31 sq. in.) in surface. They are located between the aeroplane and the horizontal rudder substantially as in the apparatus illustrated herewith by figs. 1, 2 and 3, and are revolved by the action of rubber bands.

This apparatus, provided with sleigh-runners and placed upon a common table, takes a short run and flies directly from it with a speed of 4 meters per second (9 miles per hour) in a course gently directed upward. It can be directed to the right or to the left, as desired, by means of its vertical rudder.



Now, inasmuch as the resistance of the air increases as the square of the velocity, such an apparatus, if its horizontal speed were 16 meters per second (46 miles per hour), would carry some 20 to 22 kilograms per sq. meter (4. to 4.50 lbs. per square foot), which is much better than has hitherto been achieved with plane surfaces.

This favorable result, which I owe to the combination of my elastic air screw with concave sustaining surfaces in the

* See No. 708 of *Zeitschrift für Luftschiffahrt und Physic der Atmosphäre*, Berlin, 1892.

wings, also confirms the most favorable aerodynamic formulæ deduced by Mr. Lilienthal * from his experiments with concave surfaces exposed to the wind. Moreover, Professor Well-

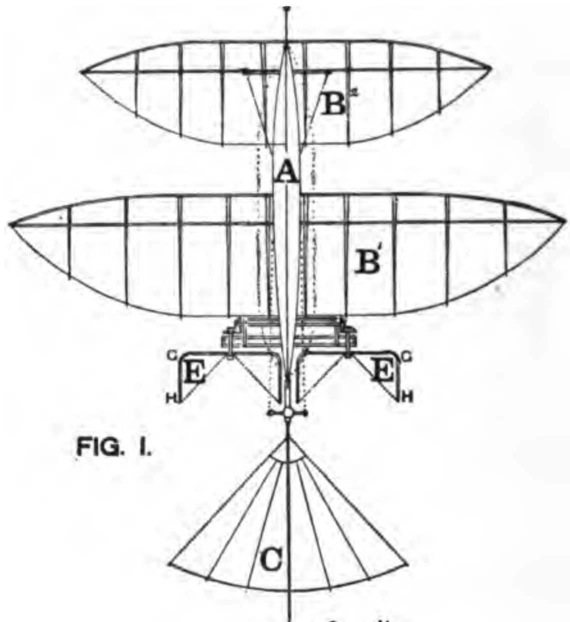


FIG. I.

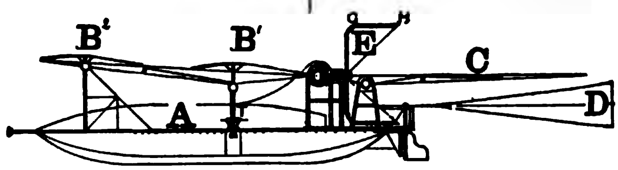


FIG. 2.

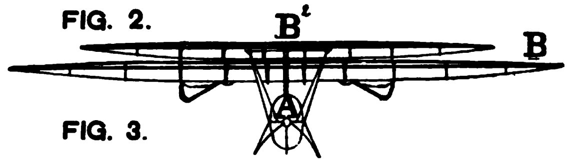


FIG. 3.

ner, of the technical high school of Brünn, has within the past year very carefully measured air resistances to concave surfaces, and has arrived at the same results as Lilienthal.

* Otto Lilienthal, *Der Vogelflug als Grundlage der Fliegekunst*, Berlin, 1889.

According to Lilienthal, the upward component of the pressure or "lift" of a concave surface inclined at a certain angle α and moved forward horizontally is given by the formula:

$$D = F\,V^2 \frac{\gamma}{g} a \cos.\,(\alpha + \beta).$$

In which F is the area of surface in sq. meters; V the horizontal speeds in meters per second; a is a coefficient depending on the angle α and the form of the surface, $\frac{\gamma}{g}$ is the weight of air divided by the acceleration of gravity, and can be assumed $\frac{\gamma}{g} = \frac{1}{8}$; a, α and β to be taken from the table herewith.

The resistance of the projection of the inclined surface, the "drift," is given by the formula:

$$W = F\,V^2 \frac{\gamma}{g} a \sin.\,(\alpha + \beta).$$

And the work necessary for propulsion is therefore:

$$A = W\,V = F\,V^3 \frac{\gamma}{g} a \sin.\,(\alpha + \beta).$$

In the following table I give some values of a and of the angles α and β derived from Lilienthal's measurements in a straight-blowing wind, with concave surfaces measuring $\frac{1}{12}$ versed sine from the chord to the arc, as given by Professor Wellner in his pamphlet on "The Possibility of Constructing Dynamic Flying Machines."

Angle α........	90°	6°	30°	15°	9°	6°	3°	0°	−3°
Coefficient a....	1·	0·91	0·90	0·90	0·85	0·80	0·68	0·55	0·36
Angle β........	0°	2°	0°	−4¼°	−4°	−2¾°	−1½°	0°	4°
Angles $\alpha + \beta$...	90°	62°	30°	10¾°	5°	3¼°	1½°	0°	1°

Having now these approved formulæ and the experience arising from the construction of my model, it becomes possible to calculate with some confidence the power required for full-sized apparatus; and for this purpose I propose to base myself upon the actual dimensions of two flying machines designed by myself, one with two fixed aeroplanes and two propelling screws, and the other with two pairs of flapping wings.

THE AEROPLANE WITH SCREWS.

This apparatus is indicated by figs. 1, 2 and 3. The covered car A, mounted on sleigh-runners, is of basket-work and of

fish-like form. It is 10 meters (33 ft.) long, 1.4 meters (4.6 ft.) high, and 0.8 meters (2.6 ft.) wide, the central cross-section being elliptical and measuring 1 sq. meter (11 sq. ft.) in area. In consequence of the pointed form of the car the resistance of this cross-section is reduced to a coefficient of $\frac{1}{5}$, so that it is equivalent to a plane 0.20 sq. meters (0.22 sq. ft.) in area. The two concave kite-like sustaining surfaces B^1 and B^2 measure together 68 sq. meters (732 sq. ft.) in area, and a horizontal rudder, C, measures 12 sq. meters (129 sq. ft.) more, thus furnishing a total sailing surface of 80 sq. meters (861 sq. ft.). The kite-like surfaces B^1 and B^2 are set at an angle of 3° with the car. The two elastic air screws E are 3 meters (10 ft.) in diameter each and measure 4.5 sq. meters (48 sq. ft.) in total wing surface. The vertical rudder D measures 6 sq. meters (65 sq. ft.) in area. As before intimated, the whole apparatus rests and glides on the ground on projecting sleigh-runners.

Now we will suppose that the apparatus has already acquired a horizontal speed of its own of 10 meters per second (22.3 miles per hour) in calm air. We then obtain for the lift:

$$D = F\,V^2\,\frac{\gamma}{g}\,a\,\cos.\,(\alpha + \beta),\text{ or inserting values}$$

$$D = 80 \times 100 \times \tfrac{1}{8} \times 0.68 \times 0.999 = 679\text{ kilograms.}$$

And for the forward resistance or drift:

$$W = F\,V^2\,\frac{\gamma}{g}\,a\,\sin.\,(\alpha + \beta),\text{ or inserting values}$$

$$W = 80 \times 100 \times \tfrac{1}{8} \times 0.68 \times 0.\,026 = 17.68\text{ kilograms.}$$

For the resistance of the equivalent cross-section of the car we have:

$$W' = F\,V^2\,\frac{\gamma}{g} = 0.2 \times 100 \times \tfrac{1}{8} = 2.5\text{ kilograms.}$$

Therefore the total horizontal resistance is:

$$W'' = W + W' = 17.68 + 2.5 = 20.18\text{ kilograms} = 44.4\text{ lbs.}$$

And therefore the necessary "work" is seen to be:

$$A. = W''\,V = 20.18 \times 10 = 201.8\text{ kilogrameters per second.}$$

Thus 679 kilograms (1,494 lbs.) are sustained with an expenditure of 201.8 kilogrameters (1,460 foot-pounds) per second. Now, as my elastic air screws with concave wing surfaces have been proved to possess a coefficient of efficiency of 50 to 60 per cent., we need for our actual power:

$$A'\ 201.8 \times 2 = 403.6\text{ kilogrameters (2,919 foot-pounds),}$$

or less than 6 H.P. to sustain the weight at a horizontal speed of 10 meters per second (22.3 miles per hour), and I calculate that the surplus power may enable us to attain 19 metres per second (42 miles per hour) with the engine of 6 effective H.P.

It may be here indicated that as there are now petroleum motors (gas engines) which are said to weigh 32 kilograms (70 lbs.) per H.P., including an "air cooler" (or apparatus for conveying the heat from the cylinder walls) and the kerosene required for about three hours' work, then the engine of 6 H.P. which we have found to be necessary would weigh 192 kilograms (422 lbs.) with three hours' supplies. If we suppose the weight of two persons to be 150 kilograms (330 lbs.), there remains for the car, for the kite-like surfaces, the air screws, rudders, etc., a residue of 337 kilograms (742 lbs.). As all the latter parts would be constructed mostly of steel ribs and covered with balloon silk, there seems to be little doubt that with the technical appliances of to-day, it is possible to construct such an apparatus and to fly with it through the air.

However, such an apparatus cannot rise by its own power directly from the ground in calm air. It needs an initial velocity to be otherwise acquired through a preliminary run or fall. But if the wind blows with some force then the apparatus needs but a short run, or even none at all if headed directly against the wind. Many large birds—the buzzards for instance—are compelled to take a preliminary run in calm air before they can get fairly under way on the wing.

THE WING-FLAPPING FLYING MACHINE.

I have also obtained favorable results with models flapping their artificial wings in direct imitation of the birds, and it can be demonstrated theoretically that such apparatus requires less work for propulsion than the fixed aeroplanes, because the air is attacked at a better angle and because the weight of the body assists in itself in producing the necessary powerful flaps of the wings.

I have described the details of the motions in my lecture on "Human Artificial Flight," delivered in Vienna on March 21, 1893,* but I want to emphasize here once more that a large bird is really always acting as an aeroplane, whether it is sailing upon the wind without muscular effort, or whether it is flapping its wings in calm air, and that the weight of the bird's body in sailing as well as in flapping flight is one of the most important elements in the actions which result in flying.

In both cases, whether passive or active, the wing is sustained by the vertical component of the pressure due to the speed and to the angle of incidence; but in the flapping action the wing upon the down stroke acts as an aeroplane gliding downward obliquely, and gains in rapidity over the "relative wind" by reason of its *negative* angle of incidence, which points below the horizon, while on the up stroke the wing also acts as an aeroplane which is lifted by the air pressure resulting from the speed produced by the down stroke, and its *positive* angle of incidence pointing forward above the horizon—

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* *Zeitschrift für Luftschiffahrt und Physic der Atmosphäre*, May, 1893.

both actions being made effective by the weight of the body. But as this waving, flapping motion is not performed at a point fixed in space, but in the yielding air, we must also consider the losses due to the friction of the air and to the resistance of the body, and for the possible loss of height on the up stroke from the sinking of the body in the yielding air.

This sinking is avoided when the wing on the up stroke has an adequate and corresponding positive angle, so that, through increased "lift," the lost height is regained or the loss of height avoided.

Now, therefore, the apparatus in horizontal flight has to encounter on the up stroke first the resistance W due to the "drift" or projection of the positive (above the horizon) angle of incidence of the wings, and, second, the resistance W' of the body. On the down stroke, however, there is no "drift" because the wing presents only its edge to the "relative wind" by reason of its negative angle, and the only resistance is W', that of the body.

As to the friction of the air, it is known to be so small that it may safely be neglected.

Now, in order to fly with flapping wings, the "drift" will have to be encountered but half the time (supposing the up and down strokes equal in duration), and the "work" required to be performed to overcome the resistance will be :

$$A = \left(\frac{W}{2} + W'\right) V,$$

V being the horizontal speed of the apparatus.

In sailing flight, the soaring bird utilizes the varying velocities of the wind, as I have explained in a separate paper, and this natural source of extraneous power saves him the muscular exertion of flapping his wings ; but in calm air or in evenly flowing air, without streaks of differing velocities, he has to perform the above estimated "work" by flapping his wings up and down.

For a practical application of these principles I have designed the full-sized apparatus shown in figs. 4, 5 and 6, which consists in two pairs of wings, elastic and concave, placed one set behind the other, as shown at A and A', and measuring in the aggregate 84 sq. meters (904 sq. ft.) in area ; also one concave fixed aeroplane, B, measuring 6 sq. meters (65 sq. ft.), and one horizontal rudder, D, measuring 15 sq. meters (161 sq. ft.). There is, besides, a vertical rudder, Q, of 3 sq. meters (32 sq. ft.) area, and the car or basket, E, 10 meters (32 ft.) long, 1.4 meters (4.6 ft.) high, and 0.8 meters (2 6 ft.) wide. The dimensions are practically the same as for the aeroplane already described, but the action is different.

In original adjustment, and on the up stroke, the wings A A' are held by means of springs at a positive angle of incidence of 8° above the horizon, but on the down stroke they automatically assume a negative angle of incidence of 8° to 15°, pointing forward below the horizon. This change of inci-

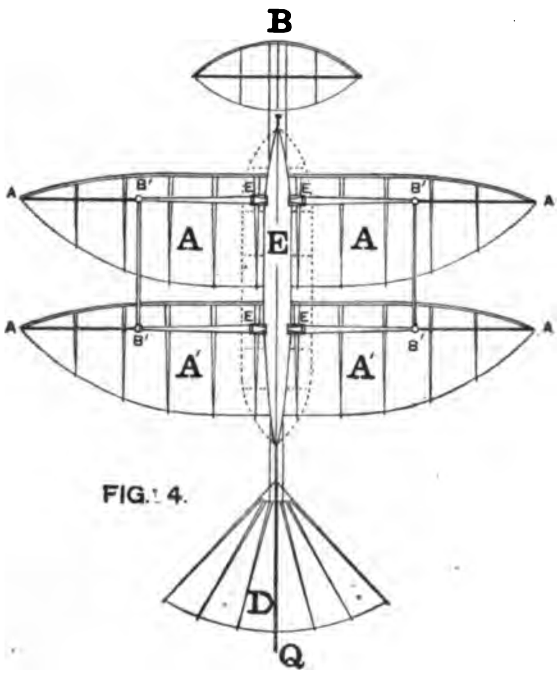


FIG. 4.

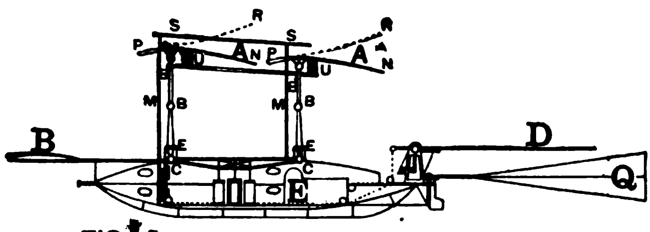


FIG. 5.

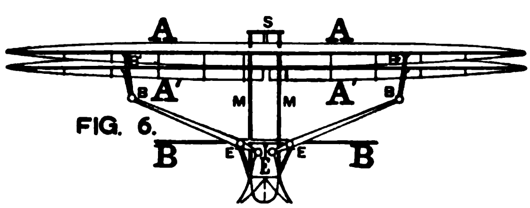


FIG. 6.

dence is produced by the increased pressure with which the downward moving wings meet the air, and which depends not only upon the "work" given out by the motor, but also and especially upon the horizontal speed of the apparatus.

The fixed aeroplane B is set and remains always at a positive angle of 3° above the horizon. I mention here an angle of 3° open at the front for the wings A and A', in order to simplify the explanation, but in practice the angle of incidence may be $= 0$. It follows that during both strokes of the wings A and A' the whole kite-like sustaining surface measures $A + A' + B = 90$ sq. meters (969 sq. ft.).

The car E, of fish-like shape, has at its thickest point a sectional area of 1 sq. meter (11 sq. ft.). By tapering the point of this car, and by properly rounding off the other parts, the equivalent cross-section of the body is reduced to 0.20 of a flat plane. The total weight of the whole apparatus, including two persons, may be estimated at 700 kilograms (1,540 lbs.), of which weight I estimate 150 kilograms (330 lbs.) for the two sets of wings A and A', together with their connecting rods b and b', and the levers of the wings e and b. This leaves 550 kilograms (1,210 lbs.) for the car and the two persons.

How importantly useful the weight of the body becomes, in connection with springs to store up energy, I have more fully explained in my paper on "Human Artificial Flight," but I may say here that part of the energy due to the pressure under the wing is stored up by springs during the up stroke, to be given out again upon the down stroke. In the apparatus here described these springs are replaced by compressed air.

The wing levers c, e, b are $3\frac{1}{2}$ meters (11.5 ft.) long, the distance c, e being $\frac{1}{2}$ meter (1.64 ft.), and the distance e, b being 3 meters (9.84 ft.). The height of the stroke is 3 meters (9.84 ft.) for the wings A and A'. This is performed at the rate of one full stroke per second, $\frac{1}{2}$ second for the up and $\frac{1}{2}$ second for the down stroke, but it may be that in practice such even, isochronous intervals of beats cannot be realized.

Before calculating the necessary lifting power and the necessary "work" for this apparatus, it must be repeated that I rely again upon the aerodynamic formulæ ascertained by Otto Lilienthal with concave surfaces exposed to the wind, which have been verified by my free-flying models.

According to these formulæ we have for the sustaining effect, at a speed of 10 meters per second (22.3 miles per hour) the following "lift":

$$D = F\,V^2\,\frac{\gamma}{g}\,a\,\cos.\,(\alpha + \beta),\text{ or inserting values}$$

$$D = 90 \times 100 \times \tfrac{1}{8} \times 0.68 \times 0.999 = 764\text{ kilograms.}$$

The resistance to forward motion is that due to the projected area of the wings A and A' at the stated angle of incidence of 3° and is given by the "drift."

$$W = F V^2 \frac{\gamma}{g} a \ sin. \ (\alpha + \beta), \text{ or inserting values}$$

$$W = 84 \times 100 \times \tfrac{1}{8} \times 0.68 \times 0.026 = 18.6 \text{ kilograms.}$$

As this resistance only occurs during the up stroke of the wings, we must divide it by two to arrive at the average power required, and we then have:

$$W' = \frac{W}{2} = 9.3 \text{ kilograms.}$$

The resistance of the projected area at 3° of the fixed aeroplane B is given by

$$W'' = F V^2 \frac{\gamma}{g} a \ sin. \ (\alpha + \beta), \text{ or inserting values}$$

$$W' = 6 \times 100 \times \tfrac{1}{8} \times 0.68 \times 0.026 = 1.32 \text{ kilograms.}$$

The resistance of the body on the equivalent section is;

$$W''' = F V^2 \frac{\gamma}{g} = 0.2 \times 100 \times \tfrac{1}{8} = 2.5 \text{ kilograms.}$$

Thus we have for the total horizontal resistance:

$$W'''' = W' + W'' + W''' = 9.3 + 1.4 + 2.5 = 13.2 \text{ kilograms.}$$

Inasmuch as the resistance is 13.2 kilograms (29 lbs.), we have for the "work" required at 10 meters per second:

$$A = W'''' \ V = 13.2 \times 10 = 132 \text{ kilogrammeters per second,}$$

or about 2 H.P., in order to drive and to sustain 764 kilograms (1,680 lbs.) through the air.

It is true that these figures possess only a theoretical value. In practice the friction of the various parts and other circumstances have also to be considered and allowed for; but it would lead us much too far to take them up at present.

Inasmuch as we must contemplate a possible speed of 25 meters per second (56 miles per hour), we will here also assume the employment of a petroleum motor of 6 H.P., which will weigh 192 kilograms (422 lbs.), and we may estimate the weight of the wings A A' with their connecting rods b b' and levers e b to weigh 150 kilograms (330 lbs.), the weight of the car to be 60 kilograms (132 lbs.), the weight of the rudders D and Q to be 40 kilograms (88 lbs.), the weight of two persons to be 150 kilograms (330 lbs.), and the weight of petroleum fuel for 10 hours to amount to 20 kilograms (44 lbs.), and we will then have:

$$G = 192 + 150 + 60 + 40 + 150 + 20 = 612 \text{ kilograms;}$$

so that while we estimate the weight at 612 kilograms (1,346 lbs.), there yet remains more than 100 kilograms of "lift" for unexpected contingencies.

And lastly, it should be mentioned that the apparatus would rest upon sleigh-runners when on the ground, that the center of gravity is placed so far to the front that in beginning to descend it becomes tilted downward in front to a negative angle of incidence of 6°, and that it cannot leave the ground in calm air by the application of its own power, but must then begin its flight from an elevation. In a wind of 8 meters per second (18 miles per hour) it can leave the ground by facing the breeze and can then fly with a horizontal speed of 25 meters per second (56 miles per hour) by the exertion of its motor.

Thus does theoretical calculation indicate that flapping wings require less "work" than an aeroplane, but it is more difficult to leave the ground in calm air with flapping wings, and the construction of the latter is more complicated and more liable to breakages. For these reasons I accord my own preference to the gliding aeroplane driven by air screws.

DISCUSSION BY O. CHANUTE.

The paper of Mr. W. Kress is chiefly interesting as indicating a direction for further investigation concerning the respective advantages of aeroplanes and of vibrating wings than as showing what can actually be accomplished with either form of apparatus, for his estimates of power required are entirely inadequate.

Even granting the accuracy of the Lilienthal formulæ which are used—and it seems difficult to do this without further confirmation, as they give a "lift" at sailing angles of 3° to 9°, of three to six times those which would be obtained with planes —it seems impossible to admit that the aeroplane calculated by Mr. Kress can maintain all the time a sailing angle of incidence of 3°. The soaring birds do not do it; they have to encounter wind gusts and eddies, to meet varying velocities of wind or of sailing, to change their course or to rise, and these almost all require increase of angle of incidence, and of consequent "drift" resistance.

For an angle of 3° Mr. Kress estimates this at 17.68 kilograms, but for angles of 6° and 9° it would, by his formula, be as follows:

Drift $6° = 80 \times 100 \times \frac{1}{8} \times 0.8 \times 0.0392 = 31.36$ kilograms.
Drift $9° = 80 \times 100 \times \frac{1}{8} \times 0.85 \times 0.087 = 73.95$ kilograms.

And an angle of 6° would seem to be as small as it would be safe to base calculations upon.

Thus we must increase the "drift" resistance of the aeroplane to at least 69 lbs., or 31.36 kilograms.

Next, Mr. Kress has entirely omitted to calculate the resistance of the framework—spars, posts, braces, etc.—and it will probably surprise a good many students of the subject to be told that this will amount to a good deal more than the resistance of the car. As the author does not give us the dimensions of the framing, we cannot calculate its resistance; but

if we assume that the front and middle spars, posts, propeller frames, sleigh-runners, etc., have an aggregate length of 80 meters and an average thickness of 0.05 meter (2 in.) with a coefficient of $\frac{1}{3}$ for their rounded form, we have for their resistance:

$$80 \times 0.05 \times 100 \times \tfrac{1}{8} \times \tfrac{1}{3} = 16.67 \text{ kilograms.}$$

Thus we have for the aggregate resistance at 6°:

Drift 31.36 + car 2.5 + frame 17.67 = 50.53 kilograms (111 lbs.), and at a speed of 10 meters' work = 50.53 x 10 = 505.3 kilogrammeters; so that, allowing 50 per cent. for the efficiency of the screws, the aeroplane requires a motor of nearly 14 H.P. instead of 6, and would require a motor of 25 H.P. if it has to sail at an angle of 9°, as seems not unlikely.

The 14 H.P. is at the rate of 106 lbs. sustained in the air per H P., which is quite as much as has ever been obtained in aeroplane experiments.

The calculations for the flapping apparatus are open to the same criticism. There has been no allowance for the air resistance to the framework, etc., and the angle of incidence of 3° assumed is probably a minimum which can only be maintained for a short time under the contingencies of flight. Indeed, if during the up stroke any energy is to be stored in a spring or in a compressed air cylinder to assist on the down stroke, it would seem that the angle must be not less than 6°; when the "lift" on the 84 sq. meters of wing surface would be 848 kilograms, or an angle more probably of 9°, when the lift would be 900 kilograms, the difference between this and the weight of 700 kilograms to be sustained producing the stored energy.

I confess freely that I do not know how to calculate the power required for flapping wings. Herr Kress calculates the drift resistance (omitting the framework), and deducts half of it because the wing descends at a negative angle and so evades the "drift" upon the down stroke; but it seems to me that in order to sustain the weight under those circumstances, and to propel. the down stroke must produce an increased pressure, and that the power required to do this must be taken into account, especially as part of it is to be stored in the spring during the next up stroke, to assist in the succeeding down stroke.

I cannot admit, therefore, that Herr Kress can sustain 764 kilograms (1,680 lbs) through the air with 2 H.P. applied to flapping wings, especially in view of the facts that Herr Lilienthal is providing the same 2 H.P. for the winged apparatus which he is now constructing, and which he expects to weigh 120 kilograms (264 lbs), or at the rate of 132 lbs. per H.P., and that the best toy bird of which I know—that of Pichancourt—in which the same wing motion of changing the angle on the up and the down stroke is produced (by mechanical means instead of automatically)—sustains but 20 lbs. per H.P.

Nor does Herr Kress seem to allow any increased weight for

the necessary greater strength of the wings to resist the constantly recurring reversals of strains due to the alternating motion. It is my own belief that this alone will preclude success with all machines in which the whole wing flaps and give the preference to aeroplanes propelled either by vibrating wing tips or by air screws, as indeed seems to be indicated by Herr Kress in his closing paragraph.

However, the question he has raised as to the respective powers required by aeroplanes and by flapping wings is an interesting one, and it is hoped that not only shall further investigations be made into the mathematics of the subject, but that there shall also be practical experiments upon an adequate scale, to test the value of the theoretical conclusions.

It is also particularly important that further experiments be made as to the sustaining power of concavo-convex surfaces. We want to make sure whether the formula used by Herr Kress is reliable. He calculates the "lift" of a curved surface of 80 sq. meters at an angle of 8° to be 679 kilograms, or 1,494 lbs., while with a plane, according to the experiments of Professor Langley, it would be but:

Lift 8° $= 80 \times 100 \times \frac{1}{8} \times 0.104 =$ 104 kilograms, or 229 lbs.—

an enormous variance, which may mean just the difference between success and failure.

NOTE ON THE ELASTIC AIR-SCREW.

By William Kress, Vienna, Austria.

I have had long experience in experiments with aerial air propellers, and I have found the best thus far to be my elastic air-screw with concave wing surfaces, as it returns a clear efficiency of 50 to 60 per cent. of the power applied.

This screw is shown in figs. 1, 2 and 3, and requires little explanation.

The rib $g\ h$ is elastic, and the propelling surface e is a loose sail which bags only during the action of the screw and then forms a concave surface. The elasticity of this screw is intended to give, automatically, to the screw at each different speed of the body which it propels, just the position of the screw wings e most favorable in pitch or angle of incidence.

There is, as is well known, for each rate of speed of the vehicle moved by the screw, a certain special angle of incidence $g\ h^1$, $g\ h^2$, etc. (see fig. 3) for the screw wing, which produces the greatest efficiency for that particular speed. To realize this the elasticity of the rib $g\ h$ must be made to correspond exactly to the dimensions of the screw and to the power of the motor.

It may here be mentioned that both for aeroplanes driven by

screw propellers and for flying machines wholly sustained by revolving screws (hélicoptères) there must always be two air screws placed as a pair, either by the side of each other or one behind the other, and rotating in opposite directions.

The arrows and the dotted lines in fig. 1 show how the air is drawn in from all sides by such a screw, and, united at a into a contentrated air blast, pushed out so as to obtain a reaction in the most favorable direction. This action can be well exhibited by holding a flame at various parts of the air-screw.

THE BALLOON AIR-SCREW.

I have constructed on the same principle an "air auger," in order to impart greater speed to a navigable balloon than has been hitherto attained.

This air-screw is shown in figs. 4 and 5. It is to be fastened to the forward point of the balloon and operated by a rotating motor attached directly on the axis of the screw at x, but the power must be supplied from the end of a very long car containing the generator.

The ribs $g\ h$ of this air-screw are also made elastic; but the arms $i\ g$ and $i\ k$ are made stiff. The surfaces e are also loose sails which bag during the action and form concave surfaces. The arrows and dotted lines show how the air currents are drawn in and directed by the screw.

When the apparatus is prevented from advancing and the screw made to rotate, a candle flame held at a shows that air is also drawn in from behind the screw in the direction of the arrow b, thus indicating that a region of rarefaction is formed behind the screw. This air screw therefore acts as an auger, having for its shaft the balloon, which follows the auger into the partial vacuum created thereby, and thus the air resistance to the balloon is considerably diminished.

Such an air-screw requires four wings, and it is also desirable that a second and smaller screw should be fastened at the

FIG. 1.

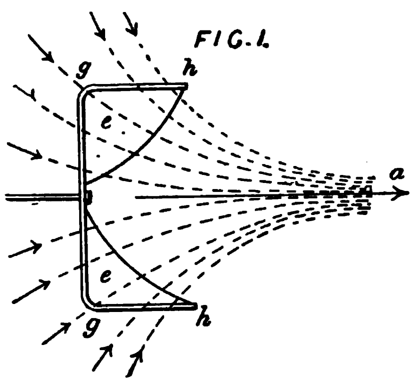


FIG. 2.

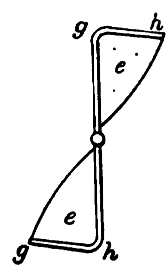


FIG. 4.

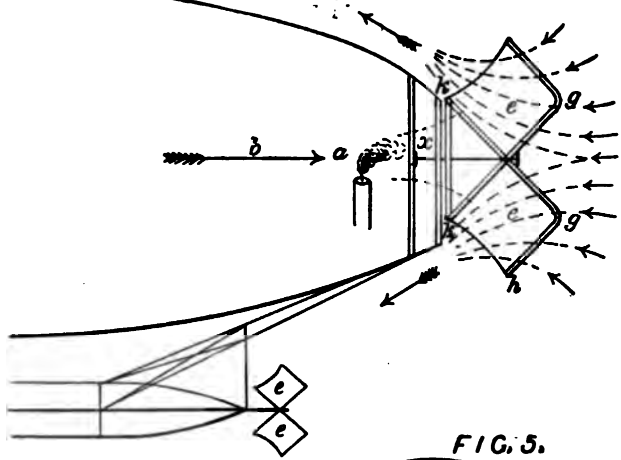


FIG. 5.

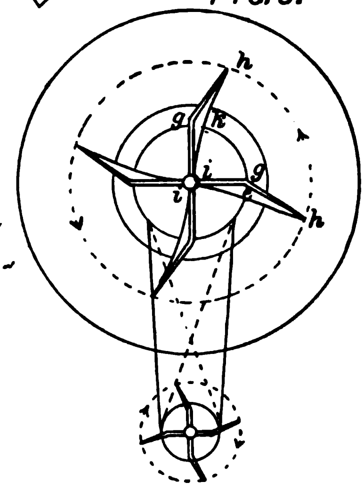
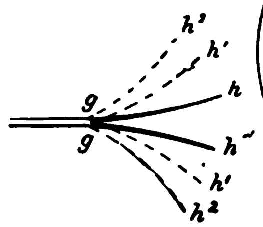


FIG. 3.



end of the car C, which screw must turn in opposite direction from the upper balloon screw.

As the present paper refers only to the air-screw, there need be no description of the balloon, which presents no new feature except that it may be mentioned that the placing and fastening of the air-screw at the point of the balloon present some serious technical difficulties.

THE ADVANTAGE OF BEATING WINGS.

By Ch. de Louvrié, Engineer, France.

In considering the possibility of compassing human flight through the air, there are two main separate features to be considered—*i.e.*, the necessity for *support* and that for *propulsion*.

The first may be obtained by mere floatation, as a ship on the water, by a balloon in the air; or, again, it may be derived from the mechanical reactions of the air by virtue of its inertia, as in the case of birds and other flying creatures.

Propulsion can only be obtained through mechanical action. Its application, therefore, requires, like support obtained from mechanical reaction, an exact and complete knowledge of the mechanics of fluids, and especially of the resistance of air.

This resistance has been determined by various experimenters; notably by Captain Thibault, in 1826, who has given us most valuable data. He measured the resistance of thin plane surfaces under motion, both at *right angles* and *obliquely* at various angles. Hutton did the same, and deduced therefrom the empirical formula that if P be the rectangular wind pressure and R the resistance or *drift* of an oblique plane, then

$$R = P \frac{1.842 \, cos. \, @}{(sin. \, @)},$$

@ being the angle of obliquity; while Duchemin represented the results obtained by Thibault by the formula

$$R = P \frac{2 \, sin.^2 \, @}{1 + sin.^2 \, @}.$$

On the other hand, I determined experimentally, in 1866, the ratio of this resistance or "drift" to the weight W of the plane moving horizontally, at various angles @, to be invariably

$$R : W : : sin. \, @ : cos. \, @,$$

and therefore

$$R = W \, tang. \, @.$$

That is to say, the resistance or drift is equal to the weight to be carried, multiplied by the tangent of the angle of incidence, no matter what may be the extent of the supporting surface.

It therefore follows that R and W are the two rectangular components of a pressure M, which is normal to the plane, and which represents the whole effect of the fluid impinging on the plane, whether the plane moves against still air or

whether the fluid moves against the fixed plane. These experiments show that this normal pressure does not vary in proportion to the square of the sine, as was heretofore taught.

As Pénaud said, the path to success was now open. The aeroplane became a possibility and its formula

$$R\ V + r\ V = (W\ tang.\ @ + r)\ V,$$

V being the velocity,
R " " resistance of the plane,
r " " " " body or car,

(W *tang.* @ V.) representing the work done to obtain support.

I submitted to the French Academy of Sciences a design for an aeroplane, which was reported upon in 1867, and I endeavored at the same time to account to myself for the beating flight of the birds by the aid of these new data.

It was clear at first sight that a certain period was occupied in the down-stroke of wing, and a certain other period, of about equal duration, in the up-stroke. The inference was that the first period was the active one, and that the second was a passive period, during which the wing acted merely as an aeroplane in supporting the weight.

The conformation of the wing also showed that its posterior portion must be tilted upward by the pressure of the air during the down-stroke, and this, too, in ratio to the speed, and that therefore it must become incurvated into a warped surface, like the blade of a screw, and act in propelling, since propulsion exists; while at the same time the wing served to sustain the weight, thus performing two functions simultaneously.

Upon the up-stroke the posterior portion of the wing becomes tilted downward, so that it presents an angle which we will call α *above* the horizon, thus acting like an aeroplane and obtaining support from the pressure due to the forward motion, while on the down-stroke, which is delivered vertically, it attacks the air with an angle directed *below* the horizon, which we will call β, the angles α and β being equal to each other.

If we call α and β the mean angle of the wing with the horizon, its stroke being vertical, then the line of motion must be oblique to the plane. If therefore we call W the horizontal component which sustains the weight, D the horizontal component which propels, U the space gone over in the unit of time on the down-stroke, and V the corresponding horizontal speed, then we have

$$D : W :: sin.\ \alpha : cos.\ \alpha,$$

and also

$$U : V :: sin.\ \beta : cos.\ \beta,$$

whence

$$D : W :: U : V.\ \text{and}\ D.\ V = W\ U.$$

That is to say, the propelling force multiplied by the speed equals the weight multiplied by the downward amplitude of the stroke.

The work done $D\ V$, in propelling, therefore represents *all* the work given out ($W\ U$) in the down-stroke. It is therefore a mere transposition of forces The two effects, the support and the propulsion, are simultaneously produced by the same organ and by the same movement; one of them must therefore be gratuitous. Now, inasmuch as the body resistance r is small, the work done ($D\ V$) is almost all converted into momentum, to be expended during the up-stroke which constitutes the *dead point* of the apparatus. Thus, on the up-stroke the wing is at the angle α with the horizon, the horizontal component $R = W\ tang.\ \alpha$ uses up the momentum, but the wing is carried upward, provided it offers no greater resistance than W, and the work of the force will then be

$$W\ U + r\ V,$$

W representing the weight of the bird.

The work to be done during the wing stroke is therefore

$$W\ U + r\ V,$$

and therefore, all other things being equal, the flapping bird only expends half as much power as the aeroplane.

But this is not all, for the work ($W\ U$) of the up-stroke, which is lost by the bird, may be saved and utilized in an apparatus for man-flight.

Thus, if the resistance to the up-stroke of the muscle which produces the down-stroke was taken up by a spring, this spring would be put under tension, and would give back the work ($W\ U$), which could be utilized upon the succeeding down-stroke. Thus suspension would be gratuitous during the up-stroke also. The motor need only furnish the work of $r\ V$.

The possibility of *obtaining suspension or support gratuitously*, without a balloon, seems astounding at first sight, but it is an actual fact, proved by experiment, and, moreover, self-evident. I have found two practical methods of availing of it on a large scale.

But there is a performance still more wonderful, long since averred, but its existence denied, because it could not be explained by existing knowledge of the mechanics of fluids, and more particularly of air. This consists in *soaring flight*, in which the sailing bird expends no force whatever, neither to obtain support nor for propulsion. And yet a general explanation is now easy: it is the wind which furnishes the motive power, provided it is sufficiently brisk, and no matter how violent.

Thus we have seen that whatever may be the angle α which a surface exposes to the wind, the resulting pressure is normal to the plane, and that it *follows that normal* instead of the line of the wind. Therefore, if the wind has an ascending trend, more or less vertical, the plane can move in any direction, even against the wind, provided it can incline in that direction and still preserve an angle of incidence α. Moreover, what-

ever may be the force of the wind, the soaring bird regulates the effects of that force according to his needs by simply changing that angle of incidence, unless he prefers to ascend vertically, in which case the elevation gained becomes so much potential energy. Thence the bird may plunge in any direction, and thus utilize gravity.

If the wind be horizontal and blowing in the wrong direction, the bird must resort to sundry manœuvres in order to gain elevation.

In the first place, the action of the wind against the body of the bird produces a horizontal component which tends to drift him back, and after a while he would no longer be sustained by the *relative* wind. He must, therefore, overcome this drift through his inertia and make up for the loss. For this purpose he constantly changes his direction and sweeps in those circlings which are so frequently observed, thus rising progressively. As he rises he finds a brisker wind, as proved by Mr. Angot, in 1889, in his observations from the Eiffel Tower.

In these circlings, if, for instance, the wind blows from the East, it is evident that the bird can, while still maintaining the angle of incidence α, at the same time incline his aeroplane toward the north and then toward the south, and thus sweep around three-quarters of the circle, gaining elevation meanwhile. During the third quarter he begins to encounter a head wind and advances only as a ship sailing *close hauled*, but in the last quarter he quite effaces the upward angle of incidence α and plunges downward. He then advances against the wind by virtue of gravity, acquires speed, and then again tilting his aeroplane upward, he rises upon the wind to his original elevation, like the pendulum, or the car in the *roller coaster*.

But horizontal winds are rare, save, perhaps, in the higher regions of the atmosphere. Near the ground the wind follows the upward slopes, with, however, steeper gradients, by reason of its inertia and its elasticity. Moreover, the atmosphere, like the sea, progresses in great waves, as may be tested both by sight and hearing. These waves are higher in the lighter fluid, and their speed is greater, and if two currents of different direction impinge on each other, they instantly produce whirls and waves which extend to great heights.

It is not necessary to describe here the various evolutions and manœuvres availed of by the soaring birds; such is not the intent of this memoir. It is only intended to give the general explanation, and to indicate that *sailing flight* can be performed upon occasion, with the same kind of apparatus which is appropriate for *rowing flight*.

But we should determine the weight which can be sustained per square meter of surface, with a wind having a velocity equal to V, and a given angle of incidence.

Captain Thibault found that a thin plane surface, moved at right angles against the air, encountered a resistance given by the formula

$$R = K S V^2 = 0.^k 115 S V^2,^*$$

S being the surface in square meters, V being the speed in meters per second.

But he found a coefficient K of $0.^k$ 120 when the wind moved against the surface.

On the other hand, the experiments of Colonel Duchemin upon fluid motions against a plane have enabled me to determine the theoretical formula of the value of the normal pressure M for oblique action, and I have found it to be :

$$M. = K S V^2 \left[\frac{2 \sin. @ \times (1 + \cos. @)}{1 + \sin. @ + \cos. @}\right]$$

This formula agrees with both the experiments of Hutton and of Thibault, and therefore confirms them. It has enabled me to calculate the dimensions of an apparatus to carry four passengers and weighing 1,760 lbs., for which I find that a sustaining surface of 1,076 sq. ft. will be sufficient, at a speed of 67 miles per hour.

With the aid of this formula it would be possible to calculate the form to be given to a solid (a car or vessel) in order to have a minimum of resistance, but marine experiments furnish us with conclusive data on this subject. Thus Dupuy de Lôme has stated that in the fleetest ships the resistance of the hull is only one-fortieth of that of the largest cross-section, and Froude (*Naval Review*, July, 1876) has stated, and with good reason, that the effects upon the run of a ship compensate the work done upon the prow. In point of fact, the fluid veins deviated at the front flow back upon elliptical curves against the rear, and press the run forward *without producing eddies.* Thus the propulsion of an aerial car, if properly shaped, would be, for a speed of 30 meters per second (67 miles per hour) and 1 square meter of largest section (10.76 sq. ft.) as follows :

$$\text{Body resistance} = \frac{0.120}{40} \times 1 \times 30^3 = 81 \text{ kilogrammeters,}$$

or in English measures 585 foot-pounds per second.

Let us add to this work the friction of the air under the wings, say, for 100 square meters, and we have :

$$\text{Friction} = 0\ 0003 \times 100 \times V^3 = 81 \text{ kilogrammeters,}$$

or in English measures 585 foot-pounds per second.

The total work of propulsion therefore, for a speed of 30 meters per second (67 miles per hour), will be but 2.16 H.P., and the suspension might be gratuitous without the use of a balloon, the resistance of which would be eight times as great.

Thus, as a summary, we find that no matter how violent

* Reduced to English measures this gives :

$$R = 0.00475\ S\ V^2, \ V \text{ being in miles per hour.}$$

the wind may be, provided it be brisk enough, it is possible to obtain gratuitously both *suspension* and *propulsion*; but inasmuch as it is necessary also to be able to proceed when the wind does not blow, it is also practicable to obtain both suspension and propulsion within reasonable limits of power.

The future conquest of the air is believed now to be only a question of experiment, and the air is a boundless sea which can be navigated in all directions.

It is the universal highway intended by God and traveled by the birds.

NOTE BY A. L. KIMBALL, AMHERST COLLEGE.

Mr. de Louvrié gives two formulæ from which he deduces a third which seems to have an important bearing on his conclusions, and which is made the basis of a discussion in the immediately succeeding part of his paper. I wish to call attention to the fact that the two formulæ which are thus combined to deduce the third cannot be simultaneously true, except when there is *no* pressure against the wings, a case which obviously does not apply to flying.

The first formula,

$$\frac{D}{W} = \frac{\sin. \beta}{\cos. \beta},$$

gives the relation between D, the horizontal component of the pressure against the wing, and W, the vertical component of pressure, and is true if the resultant pressure is perpendicular to the wing, which, experiment shows, may be assumed to be the case for practical purposes. That is, when the wing strikes the air the direction of the pressure is perpendicular to the wing, and so if the wing is inclined at an angle β to the horizon, there is a pressure, D, urging it forward, and at the same time an upward pressure, W, resisting the downward motion of the wing, and the relation between these two is correctly given in the formula above cited.

But the second formula,

$$\frac{U}{V} = \frac{\sin. \beta}{\cos. \beta},$$

where U represents the velocity with which the wing is moving downward, and V its velocity forward in a horizontal direction, expresses the fact that the wing is moving through the air *edgewise*. This is easily seen by reference to the figure —U and V are represented in the ratio given by the above equation—and it is evident that if the wing moves forward a distance V in one second and in the same time moves down a distance U, that at the end of the second it will be in the position A', having traveled edgewise from A to A'.

So it is evident that whenever the relation between its forward and vertical velocities is expressed by the formula

$U = V \, tang. \, \beta$, above mentioned, the wing at that instant is actually moving edgewise through the air, although the body of the bird may be moving horizontally forward. This is so whether the wing is taking a down-stroke, its front edge being lower than the back, or an up-stroke, with its front edge higher than the back.

Now, clearly, when the wing moves edgewise through the air there is *no* pressure against its flat surface, and so in this case the two component pressures D and W, above referred to, will each be zero, and there will be no propulsive force and no sustaining force.

To produce a *sustaining* and *propulsive* force on the down-stroke of the wing, the velocity U must be *greater* than $V \, tang. \, \beta$; while that of the wing may act as an aeroplane on the up-stroke and develop a *sustaining* and *resisting* force, its upward velocity must be *less* than $V \, tang. \, \alpha$, where α and β are the inclinations of the wing, to the horizon in the two cases respectively.

STABILITY OF AEROPLANES AND FLYING MACHINES.

BY A. F. ZAHM.

I HOPE that this paper will be regarded rather as a speculation intended to draw forth information from others than as a presentation of final or satisfactory conclusions. The ideas here expressed have been suggested by experiments with a variety of models for the most part launched freely in space without propellers. If I shall succeed in evoking more mature ideas from others who may have pursued this subject to some definite issues, the purpose of the paper will have been realized.

STABILITY OF AEROPLANES.

To simplify the study of the stability of flying machines, it will, perhaps, be best to consider the sustaining surface entirely apart from the propelling surface. It is, of course, not of necessity a separate part. A machine might be designed in which the propeller and sustainer were identical, as, for example, a machine containing no winged surface except a screw; but in all the more successful types of the present time the parts are decidedly separate and distinct. The machines of Tatin, Phillips and Maxim are propelled by a screw and supported by an aeroplane; that of Hargrave is propelled by a pair of flappers and sustained by a plane, while the large birds, the most successful of all flyers, are propelled chiefly by the outer parts of their wings and supported by the parts nearer the body.

The sustaining plane may then be considered independently of the propeller, and since the latter serves only as a force act-

ing in the direction of flight, it may be disregarded for the present, leaving for consideration the forces of inertia and gravity of the machine and the atmospheric pressures.

After studying the flight of a free aeroplane urged by the forces of gravity and inertia, we may add the force derived from a propeller, and extend the discussion to flying machines proper.

Stated in all its generality, the problem is to devise a simple and self-contained aeroplane which will (1), when launched in any manner, automatically head to the wind and move rapidly forward; (2) when displaced or overturned promptly recover its position of equilibrium; (3) maintain a prescribed and uniform average course and position during flight.

TRANSVERSE STABILITY.

The problem of the transverse stability of aeroplanes seems never to have offered much difficulty; for no one could study long without discovering at least the rule of placing the center of mass below the center of buoyancy, and making the sustaining surface more or less trough-shaped. This might be learned from a few experiments with paper models. It has always appeared in the figures of the great sailing birds. It might be suggested by a boat balancing itself on the water. The rule has therefore been observed by most designers of flying apparatus heavier than the air.

But though it may be evident enough that transverse stability is promoted by making the sustaining surface trough-shaped, it is not apparent what form of cross section is the most efficient for sustentation and equilibrium combined. The circular form is certainly neither favorable to stability nor to support. The simple V form is, perhaps, the easiest to pattern after. It has been recommended by writers and has given fair satisfaction in trial, but it has never been proved the most effective. It has been adopted by experimenters, but not by Nature in her great soaring birds. It will, therefore, be necessary to study more minutely the conditions and requirements before deciding upon the best form of transverse section of an aeroplane.

If a simple rectangular surface, which has been adjusted for all but lateral stability, be carelessly projected forward in space, it will be found to rotate about its longitudinal axis at the slightest puff of wind or other disturbance. If, then, it be given the customary V form, or, worse still, that of a circular ark, it will promptly recover from any sudden displacement, but will rock like a boat without a keel, exhibiting a tendency to revolve about an axis through its center of curvature, or through the center of a curve which approximately coincides with the cross section of the model, as shown in fig. 1 (*A*). The small space between the bounding circles of the aeroplane section in fig. 1 shows that it cannot meet much resistance to gliding tangentially; and so the slight keel *k* added below is seen to treble the section opposed to such gliding.

We thus discover, as in boat-building, two conditions to be provided for: (1) That of stability against inversion · (2) that of stability against rocking. There is also a third requirement —viz., that of greatest buoyancy consistent with simplicity and strength.

Barring the effects of inertia, the stability against inversion of an aeroplane depends upon the nature of the moment of two forces—that of gravity, acting downward through the center of mass, and that of the resultant pressure of the air against the surface, which acts from below upward. If these forces be always in the same vertical plane, the equilibrium will be neu-

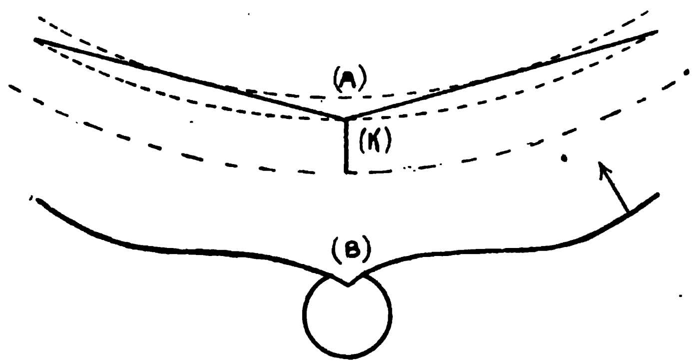


Fig. 1.

tral; if not, the equilibrium will be stable or unstable, according as the moment of the forces tends to right the surface or to further invert it when once displaced. Evidently the greater such moment the greater the consequent stability or instability.

To apply this reasoning to an aeroplane of a particular form of transverse section, let us begin with the straight line, which we will suppose to be the cross section of a perfect plane whose center of gravity is within the surface. As the surface rests level on the air, the resultant effort of the air and of gravity lie in the same vertical line, and the body remains in neutral equilibrium. If it receives a slight inclination to one side, it will tend to glide in that direction, and there will be no prompt and efficient moment set up to restore it to the level position. But suppose the surface to assume the V shape, its sides forming a large diedral angle; then directly the plane is tilted or disturbed by a puff of wind, its depressed half will receive more support than the other, and at once restore the body to its level position. For this reason the simple V form has found favor with many experimenters who seem not to have desired anything better.

It is clear that the stability of the V form increases as the diedral angle diminishes; but when the angle grows very small, the stability is lost. What angle gives the greatest stability I am not prepared to say, though I should conjecture that it must be one less than 90°, the angle at which the lower half receives its maximum support, while the other is in a position of no support. The question is perhaps more curious than profitable, since we cannot afford to sacrifice support to stability by adopting a very acute V form of cross section.

A further consideration of the requirements of the aeroplane suggests an improvement on the plain V form of transverse section. If the diedral angle be made very large and the outer edges of the surface be turned upward, as indicated in fig. 1 (*B*), there results an increase of support without lack of stability. It is evident that the moment of a unit of surface of an aeroplane is greater at the outer edge than elsewhere, because of its greater lever-arm. It is better, therefore, that the outer edges of the aeroplane be turned upward to maintain equilibrium, while the inner surface remains flat or concave for greater support. For the same reason the distance from tip to tip should be as great as practicable.

We have now only to modify the form so as to prevent rocking or gliding to and fro sidewise. This can be effected by providing some form of keel, as indicated in fig. 1 (*B*).

We have thus approached step by step, and unintentionally, almost to the outline of a bird. If the center of mass be placed below the base of the wings it must greatly increase the stability, and in any actual machine that would be the natural location of the motive power or chief burden.

It is doubtful whether a bird or a small flying machine could possess automatic stability unless the proportion of sustaining surface were abnormally small. It is maintained by good authority that even the eagle, the buzzard and the great seagulls are obliged to constantly balance themselves in a fickle breeze. And what exquisite organs they have for this purpose!—nerves to feel the minutest pressures, and muscles to alter, in response, every portion of the sustaining surface. This alone accounts for their incomparable ease and grace of motion when sailing on extended wings—a grace which implies all the skill of perfect acrobatism. It is encouraging to reflect that stability increases with magnitude, and hence that possibly the great flying machines of the future may be made self-equilibrating.

I have mentioned the advantage of placing the center of mass below the center of surface; this has also its objections. While the stability against inversion is increased, the stability against rocking is sacrificed. The aeroplane so constructed may not easily overturn, but it will sway to and fro with a pendular motion. This, when lateral, is very objectionable; when fore and aft it is fatal to uniform progress, as we shall see in studying the longitudinal stability of flying machines. We shall then see that the center of mass cannot be lowered with impunity.

I may notice a similar antagonism between the forms for

sustentation and for stability. An aeroplane whose transverse section approaches that of a boat gains in stability, but loses in buoyancy. The convex outline is better for one purpose, the concave for another. In this respect it seems more difficult to design an aeroplane than a boat; for while in a boat the forms of advantage conspire, in the aeroplane they clash. I may mention one instance in which they conspire—*i.e.*, when the spread or distance from tip to tip is very great; for long, narrow wings are most efficient both for support and for equilibrium. What may be the most favorable outline of transverse section for stability, steadiness and buoyancy combined I submit as unsolved.

A word as to the stability of the compound aeroplane. This consists of many planes superposed and so spaced as not to interfere one with another. It might well have its supporting planes or slats set obliquely for the reasons already given; but this has not generally been observed by experimenters, though it seems to me quite as essential in the compound as in the simple aeroplane. It is true the vertical stays could be formed of slats or planes which, if the machine were tilted, would receive sufficient side pressure to right it after the manner of a top keel, even if the supporting slats were not set obliquely; but they would act less promptly and effectively than the oblique sustaining slats. Such vertical planes would tend to counteract both rocking and inversion.

STABILITY ABOUT A VERTICAL AXIS.

If an aeroplane adjusted for lateral and longitudinal equilibrium be launched freely in space, it will generally be observed to turn about its vertical axis as it advances in its forward course. If the aeroplane have not a vertical keel or its equivalent, the path followed may be nearly a straight line, since the inertia urges it straight forward, and the side of the plane meeting least resistance will move in advance of the other, thus causing the plane to progress sidewise along its course—a very objectionable behavior. If, however, the aeroplane be provided with a proper keel it will always advance headforemost, and the path followed will be a large curve, approximately a circular arc. This curved path may be due to either of two conditions: one side of the plane may be heavier than the other, or one may meet more resistance than the other. In either case a couple is formed between the urging and opposing forces with a deflecting effect like that of a vertical rudder.

The straightness of the course described will, of course, depend upon the perfection of symmetry of the aeroplanes; and I doubt whether one can be constructed of such perfect proportion as to follow a straight course except under the guiding hand of a living pilot, or, perhaps, of a controlling magnet. However, the same may be said of a boat. In a disturbed element neither a boat nor a flying machine may be set to follow a direct course without guidance.

An aeroplane which has neither a vertical rudder nor a keel, or its equivalent, will not only yield to the moment just described, but may actually turn so as to move sidewise or tail foremost. Hence both a keel and a rudder turning about a vertical axis seem to be as essential to a flying machine as to a boat. It is true an aeroplane might be guided to the right or left by shifting its center of gravity, but this is only another way of stating the same need.

We will see, in studying the question of longitudinal stability, that a rudder to guide up and down is also essential, and is actually supplied to all the birds in the form of a tail. To meet, then, the requirements of turning right and left and up and down it seems necessary to supply all self-equilibrating machines with a double rudder.

The double rudder is found quite commonly in nature. The blackbird, for example, to steady its flight, almost invariably distends its tail in the form of a V. Most birds, in steering, rotate the tail to some extent when they wish to exert a lateral pressure. All resort to the device of shifting their center of gravity or of increasing the support or resistance of one wing more than the other. The exact manner of effecting these manœuvres has been abundantly described by various writers, and need not now engage our attention.

LONGITUDINAL STABILITY.

We have considered the conditions of equilibrium and stability of an aeroplane about two axes, and found them comparatively simple; but it is otherwise with the third axis. The problem of providing for longitudinal or fore-and-aft stability is one of the most serious which the present aviators have to encounter. The problem might be less difficult if we had an artificial propelling force to dispose of; but that would transform the aeroplane into a flying machine whose stability will be considered later.

Let us first try to obtain a solution for the aeroplane, then for the aeroplane type of flying machine and whatever other types may suggest themselves.

It will be well to remember, in these discussions, (1) that the force of gravity always acts vertically; (2) that the force of inertia always acts through the center of mass, and equals the product of the mass of the aeroplane into its acceleration; (3) that the center of pressure of the air against the aeroplane, neglecting hull resistance, varies for different angles of impact, according to Joessel's law; (4) that a curved surface tends to follow a course of coincident curvature.

To simplify the treatment of the subject it will, perhaps, be well to present it in several particular propositions. First, let it be required to find the conditions of equilibrium of an aeroplane gliding down an inclined course with uniform velocity.

The force of inertia is zero. Place the center of gravity in advance of the center of support and incline the rudder below the line of progression, as shown in fig. 2. It will be seen,

from an inspection of this figure, that when the forces at play are in equilibrium for any particular velocity, then, if the velocity be slightly increased, both N and n, the sustaining and guiding pressures, will be increased, while G, the force of gravity, remains constant; hence the aeroplane, under this increased velocity, will rise in front and begin to pursue a less sloping course. But as it glides at a greater velocity on a less sloping course the head resistance will be increased, while the urging component of gravity, which should equal it, is diminished. The increased speed, therefore, cannot be maintained, and the aeroplane will return to its normal position. Conversely if the speed were diminished below its normal value, the plane would move down a more sloping course, acquire an increase of speed, and mount to its normal position. Thus for a slight displacement in either direction the plane returns to its initial position, which must consequently be one of stable equilibrium.

It can easily be shown that the normal course of the aeroplane just described lies between the horizontal and the vertical directions—*i.e.*, that it must be sloping. However, this is self-evident.

Although the conditions just presented secure stable equilibrium, according to the definition of the term usually given in mechanics, they do not secure perfect steadiness. The reason is obvious; for when a small model of this kind is disturbed in its path it recovers too promptly, and thus passes, like a pendulum, to a point beyond its normal position. The path described by the aeroplane must, therefore, be a wavy one instead of a direct one, and can be made direct only by damping such pendular movement, or by some device for keeping the center of pressure directly over the center of gravity, thus eliminating the need of a rudder pressure and the consequent deflecting moment caused by it.

Damping may be furnished by the friction of the hull of the aeroplane or by trailing attachments, such as the fluttering tails of kites, etc.; but these are objectionable because of their consumption of energy. I know of no special device for keeping the center of resistance constantly over the center of gravity. We have then only pendular equilibrium, not perfect stability, except in so far as this may be secured by the use of a horizontal keel, to be noticed presently.

In the design of fig. 2 I have placed the center of mass in the surface of the aeroplane; if it were placed below the surface, the equilibrium would be more stable, but the tendency to pitch, especially during accelerated motion, would be increased. In the case of lateral rocking the remedy was found quite simply in the form of a keel whose resistance damped the rocking; but a keel may not be employed to oppose pitching in quite the same way. The keel which corrects lateral rocking opposes all lateral movement, whether of rotation or of translation; but the keel which dampens pitching must oppose only rotation, while it favors advancement in a straight line. Such qualifications are possessed by a plane surface of great

length passing lengthwise through the aeroplane, and parallel to the plane of its transverse and longitudinal axes. Hence a very long body plane, as observed in Hargrave's models, may supply the required steadiness, or the same may be imparted by a tail or keel of great leverage.

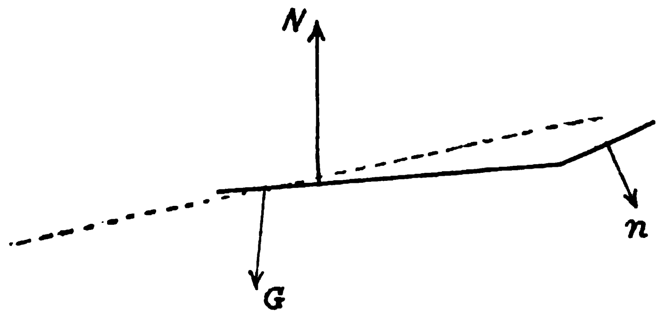


Fig. 2.

It appears, then, that when an aeroplane has been properly adjusted for equilibrium a vertical keel will steady it against rocking and wheeling, while a horizontal keel will steady it against pitching. It may be noted that the further such keels or fins are located from the center of mass, the greater their leverage and consequent effectiveness will be.

As to the disposition of the guiding surfaces, we may note that the center of pressure of the vertical keel should be in the vertical gravity axis of the aeroplane, and sufficiently above the center of mass to secure an effective equilibrating moment. The center of pressure of the horizontal keel should be well abaft the center of mass to afford a long arm to oppose the moment of the inertia of the aeroplane during accelerated motion, as at starting and stopping, or when pitching occurs from puffs of wind, etc. The vertical rudder or tail of the aeroplane should have its center of pressure in the longitudinal gravity axis, so that there may be no lever arm to cause transverse rocking when the rudder is applied to right or left. However, when this rudder is turned, the reacting pressure of the keel, which has a lever arm, will tend slightly to tilt the aeroplane, but always in the proper direction to conspire toward describing the course corresponding to such turn of the rudder.

Let us now place the center of gravity between the center of surface and the center of pressure for a given angle of flight, as in fig. 3. With this disposition of the centers of gravity and of wing pressure the tail or rudder pressure must be from below. Hence the moment of the wing and tail pressures are opposed; and if the velocity of the aeroplane in its normal

course is increased both opposing moments will be increased together, so that the aeroplane will not have the same tendency to rotate about its center of mass as it had in the last example. It will consequently descend on a less sloping course if proportionately weighted, and would tend to preserve a position

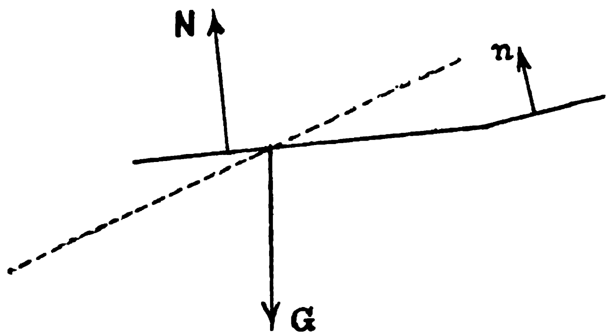


Fig. 3.

always parallel with itself except for this reason : the course becoming less sloping, the angle between the aeroplane and the course must become less and less ; owing to Joessel's law, the center of pressure must advance and cause a moment tending to raise the forward edge of the plane. This design, then, has the defects of the other, except in a less degree, because the moments of wing and tail pressures conspire in fig. 2, while they oppose one another in fig. 3.

We need not consider the case in which the center of gravity coincides with the center of surface, because it is well known that such a design possesses neither equilibrium nor stability, but tends to rotate backward as it advances forward.

For these aeroplanes (figs. 2 and 3) there seems, then, to be no remedy against pitching apart from keel and damping devices, except to keep the center of pressure from varying or to make the center of gravity vary with it. The first of these can be nearly effected by making the wings very narrow, as in Phillips' machine, where they measure about 2 in. in width, so that the center of pressure can vary but a fraction of an inch. The couple formed by gravity and the sustaining pressure in the Phillips or Wenham design of aeroplane must indeed be one of very slight moment. It certainly could not cause sudden pitching, especially if the aeroplane were provided with a tail or keel of good leverage.

These considerations incline me to think very favorably of narrow wings and keels of long leverage.

It has been maintained that the longitudinal section of the aeroplane surfaces can be so curved that the center of pressure will not vary with either a varying angle of flight or a varying velocity; but I know of no one who has proved this either analytically or experimentally. It has been said also that the center of pressure of a bird's wing is nearly fixed, owing to its peculiar curvature, but this has not been proved.

The keeping of the center of gravity always under the center of pressure by shifting its position may be effected by a variety of special devices, as by shifting weights, etc., and need not detain us.

The means thus far considered, though they do not secure perfect steadiness, seem nevertheless to afford sufficient stability to render flight practicable, especially when we remember that perfect steadiness is not required of an aeroplane more than of a boat. All boats rock as well as all soaring birds, notwithstanding that the greater viscosity of water should make the boats steadier than the birds in passive flight.

We have provided for the stability of an aeroplane of uniform velocity, and this seems to cover all the conditions of flight of a free plane, because its weight remains constant and the urging component of gravity tends to assume a definite direction and magnitude and thereby to impart a uniform velocity. When the speed is increased or diminished by gusts of wind, etc., the aeroplane oscillates and presently recovers its normal position, as does a boat on the water.

All that has been said evidently applies to the aeroplane type of flying machines; hence it appears that in passive flight equilibrium and a reasonable stability are feasible without a pilot, and considerable steadiness attainable with a pilot.

It need hardly be added that an aeroplane fulfilling all the requirements thus far set forth will, when overturned or disturbed in any manner, promptly right itself, head to the wind, and move forward on a fairly uniform course.

STABILITY OF FLYING MACHINES.

The problems of the automatic stability of flying machines of the aeroplane type are identical in most respects with those we have been examining. It differs only by the introduction of an additional force—that of the propeller—whose line of action will in general lie in the vertical plane through the longitudinal axis of the machine. During regular flight this force may be constant in magnitude and direction with reference to the machine; but the necessarily varying conditions of flight will require the propelling force to vary frequently in magnitude if not in direction also.

Let fig. 4 represent such a machine in uniform horizontal flight, the tail being double, the narrow supporting planes superposed, as indicated by the parallel lines, and the center of gravity well below the center of support. If the propeller act in the line R of resistance to progression, the machine will continue to fly with uniform speed and preserve its equilib-

rium. If the speed be sufficient, the path of flight will be level; if too fast or too slow, the path will slope upward or downward accordingly. Thus it appears that without using the rudder or shifting the center of gravity, which is the same in effect, a level path can be followed at one particular speed.

If the supporting planes be so pivoted that their inclination can be altered, the machine may fly at very swift or at comparatively slow speeds and yet find sufficient support to maintain a level course. On this principle were constructed the most successful sailing models with which I have thus far experimented. Their wings were shaped very like a bird's wing, with a rather strong arm whose torsional elasticity was enough to allow the wing to slightly rotate with the varying pressure. The wing thus automatically adjusted its angle to the varying velocities of flight, and so exhibited a very uniform and graceful motion.

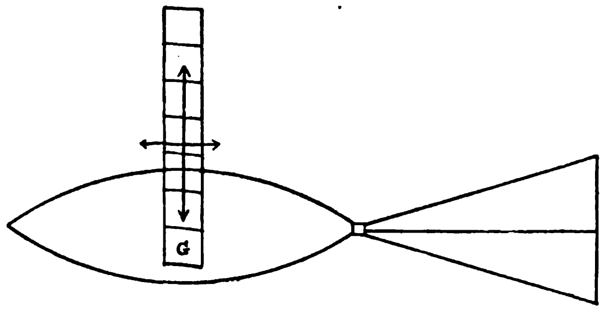


Fig. 4.

To insure lateral stability the sustainers might, of course, form a diedral angle, as shown in fig. 1, or the machine might be provided with a vertical keel. If the sustainers were pivoted it would be well to have them quite narrow, so that the center of support might not seriously alter for varying angles of flight, thus demanding the use of the rudder. The body of the machine should have sufficient keel to preserve a steady, straightforward course. Finally the center of gravity should be low enough to insure stability.

Should such a machine be struck by a sudden head wind or side wind, pitching and rocking would ensue; for these can in no manner be prevented in a flying machine more than in a boat riding a torrent. It may be observed also that if the pull of the propeller should suddenly cease, the unbalanced resistance of the machine would form a couple with the force of inertia. This couple would cause pitching, tending to make the machine rise in front and rotate backward; but the moment of gravity and of the rudder pressure would oppose such disturbance. The machine would thus presently assume a

definite inclined course, and glide down the atmosphere as an aeroplane.

The preceding remarks have had reference to a machine whose wings are composed of many sustainers or supporting planes; but they will, evidently, apply as well to machines whose wings are composed of single sustainers, as in the Tatin, Maxim and Hargrave models.

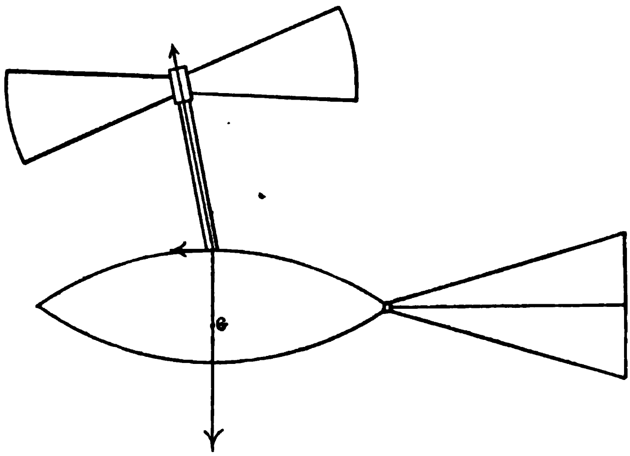


Fig. 5.

Let us now consider the stability of a machine supported by a propeller alone, without the aid of sustaining wings. Such a one may be represented by fig. 5, in which the shaft of the propeller is directed upward at a large angle instead of horizontally, as in the preceding figure. The propeller may, of course, be composed of twin screws or a pair of flappers, or any other device that will exert a balanced pull.

For the present let us suppose that the pull is balanced and that the propeller shaft is attached pivotally at the point of intersection of the vertical gravity axis and the line of resistance to flight. Let us also have a double rudder, sufficient keel, and the center of gravity placed well below the center of support. If, then, the propeller shaft is set vertically, the machine will rise directly upward, and if the shaft is inclined slightly to the front the machine will move promptly forward at a speed depending upon such inclination, and all the forces will be in equilibrium except that of inertia, which for uniform flight has no value.

Now let us assume that a balanced pull is not maintained. For the sake of simplicity it may be found well to employ a single screw, as shown in fig. 5. Such a machine could, on

starting, be made to rise and fly rapidly forward, and, if the rudder remained straight, the torsional reaction of the screw would cause the machine to turn steadily around, thus describing a large circle in the air. A slight inclination of the rudder would then be sufficient to guide the machine straight forward, or even to turn it in the opposite direction. If the requisite power could be obtained, a machine of some such design would, perhaps, be found the most convenient for man's first essays at active flight.

It is evident, then, that machines supported only by the force of their propellers may be designed to possess excellent stability and fly at very high velocities.

CONCLUSION.

To recapitulate, it has been shown for an aeroplane, that (1) automatic lateral stability may be secured by locating the center of mass low down and placing the sustaining surfaces at a diedral angle, or by placing a vertical keel above the center of gravity, like the vertical fin on the back of a fish; (2) automatic stability about a vertical axis cannot be obtained at all except by some special appliance, such as a controlling magnet; (3) automatic longitudinal stability may be attained (*a*) by placing the center of gravity before the center of wing lift while the rudder is inclined below the line of flight, or (*b*) by placing the center of gravity behind the center of wing lift while the rudder is inclined above the line of flight, or (*c*) by some device for automatically varying the inclination of the sustainers or shifting the center of gravity; (4) lateral steadiness—*i.e.*, against rocking—may be secured by the use of long wings, or a fin-like keel of long leverage; (5) steadiness about a vertical axis by use of a vertical keel; (6) longitudinal steadiness by use of a horizontal keel.

These ends could probably be attained very well by mounting two compound aeroplanes on a long backbone, somewhat after the manner of the Hargrave cellular kites, and adding a compound rudder to the whole. The sustainers and horizontal rudder would perform the part of a horizontal keel, while vertical slats in the aeroplanes or a single sail stretched along the top of the machine would answer for the vertical keel. The flight ought, therefore, to be quite steady. If the inclination of the sustainers front and back could be altered independently, it might be feasible for a pilot to preserve the equipoise of the machine even when its center of gravity was frequently shifted, as by the moving of passengers to and fro.

The term keel, as used in this paper, means a fin-like surface designed to guide and steady without opposing the progress of the vessel. Its function is in some respects like that of the centerboard of a yacht, though not identical. Perhaps "fin" would have been as appropriate; but the word has been used with the above meaning by a number of writers, and notably Mr. Chanute in his articles in the AMERICAN ENGINEER.

My inquiries in this paper have been directed chiefly to free

aeroplanes. They will, for the most part, apply to propelled aeroplanes also; and I think that the addition of a propeller will in general be found to impart steadiness; so that the equipoise may be more easily maintained in active than in passive flight.

We have been considering the question of automatic stability in so far as it may be secured in the construction of the craft itself, apart from a pilot or special equilibrating devices. The application of the latter would give exercise to an infinite amount of ingenuity, and would, perhaps, best be left to the fancy of the individual inventor. One curious design, however, occurs to me, which, since I have not seen it described elsewhere, may be worth a moment's notice.

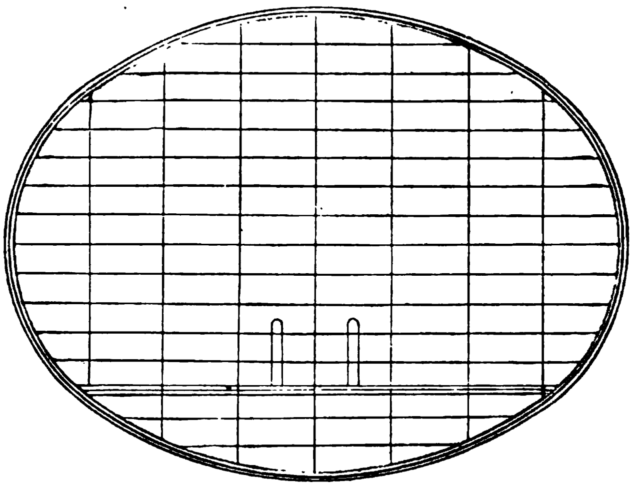


Fig. 6.

Suppose a Phillips' machine to be provided with a double tail and to have a vertical fin extending longitudinally along its entire length, well above the center of gravity. These would steady its flight and promote stability. Suppose, also, that its sustaining slats were pivoted so that a pilot could at pleasure change their inclination on the right and left side independently. He could then set the engine for a desired speed, sweep forward along the earth with the sustainer slats horizontal, and, at will, mount into the air by giving the slats an upward inclination. Once in the air he could raise or lower the machine by slightly changing the angle of the slats; he could wheel to right or left by giving one set of slats a little

different slope from the other; he could arrest all pitching, rocking and wheeling by a slight counter movement of the sustainers. It would be necessary, of course, to preserve a rapid forward motion; for it is a peculiarity of the compound aeroplane that if it comes to a standstill in the air it will drop plumb down with a frightful plunge until it acquires headway.

I have indicated in fig. 6 the front view of an aeroplane with pivoted slats. Suppose the frame to be circular or oval, and to be stiffened, like a tennis racket, by steel wires or ribbons stretched across it, as shown. Let the horizontal lines represent narrow sustaining surfaces made of wire covered with silk or of thin slats or of steel ribbons. Suppose these narrow sustainers to be elastically pivoted at their front edges and connected at their rear edges with vertical guide wires running down to two eccentrics. The pilot holding the levers shown could, by a slight turn, rotate all the slats on either side of the aeroplane, and that without the exertion of much force.

I doubt not that an aeroplane of some such design could be efficiently made entirely of steel, and, when in full flight, yield to the slightest turn of its handles as readily as a bicycle.

It is doubtful whether, in the experimental period, we ought to strive for perfect automatic stability. It seems to me more practicable to begin with small machines which a single man could manipulate without great exertion.

I would suggest three methods of learning to rise in the air: (1) By direct lift, as when a vertical screw is used; (2) Lilienthal's method—that is, by learning to ride the aeroplane before the motor is added; (3) by gliding rapidly over a field of ice or other smooth surface, not allowing the machine to rise more than a few inches till the pilot has acquired skill and confidence.

FLYING MACHINE MOTORS AND CELLULAR KITES.

BY LAWRENCE HARGRAVE.

INTRODUCTION BY A. F. ZAHM.

It will appear evident that Mr. Hargrave, in preparing this brief but valuable paper, presupposed that the members of the Conference were familiar with his former experiments and contributions to the science of flight. The complete details of his labors during the past decade may be found in the proceedings of the Royal Society of New South Wales. A comparatively full and quite appreciative account is likewise given in Mr. Chanute's new work, "Progress in Flying Machines," and in the columns of the AMERICAN ENGINEER AND RAILROAD JOURNAL for May, September and October, 1893. For the convenience of the reader it may be well to recall the chief facts therein presented.

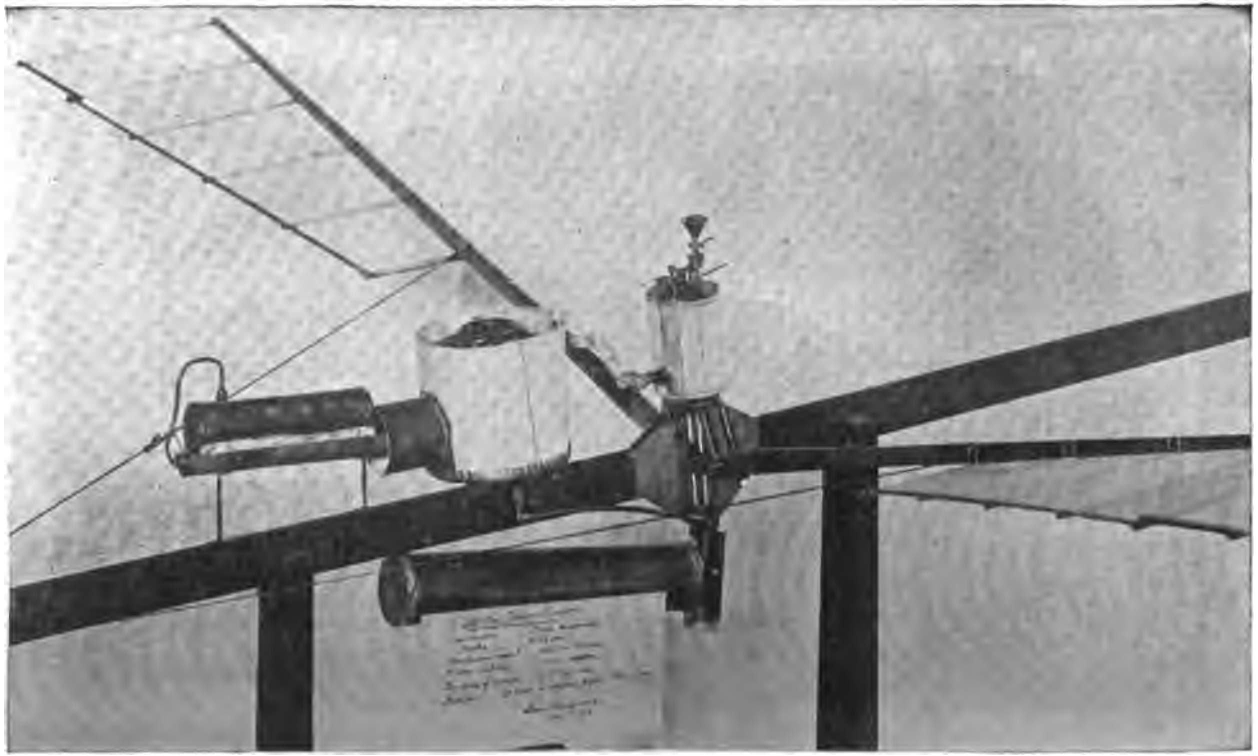


PLATE I.—HARGRAVE STEAM-ENGINE OF NO. 18 FLYING MACHINE.

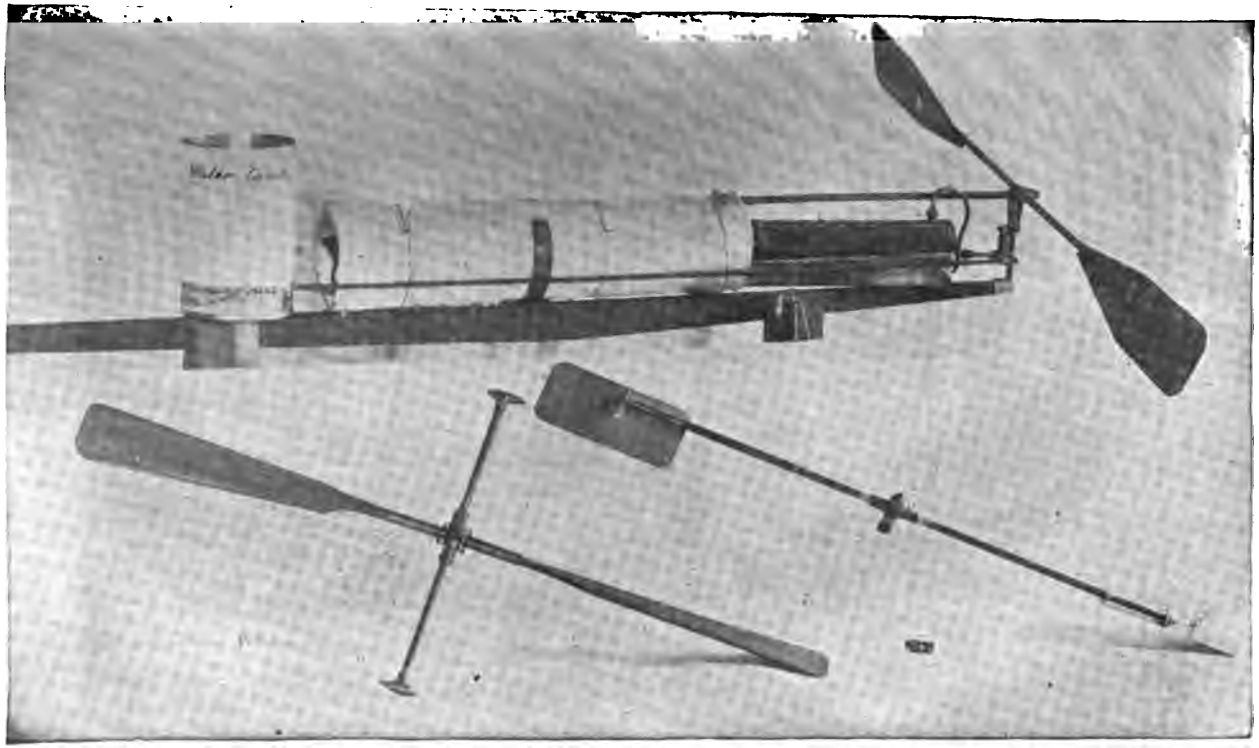


PLATE II.—HARGRAVE: VARIOUS SCREW-MOTORS FOR FLYING MACHINE NO. 18.

At the beginning of the past decade Mr. Hargrave constructed a great number of flying models of the general type shown in the *American Engineer and Railroad Journal* for May, 1893. These were propelled by either a screw or a pair of flappers, and sustained in the air as usual by the kite-like lift of the body plane. The propellers were actuated by springs—at first

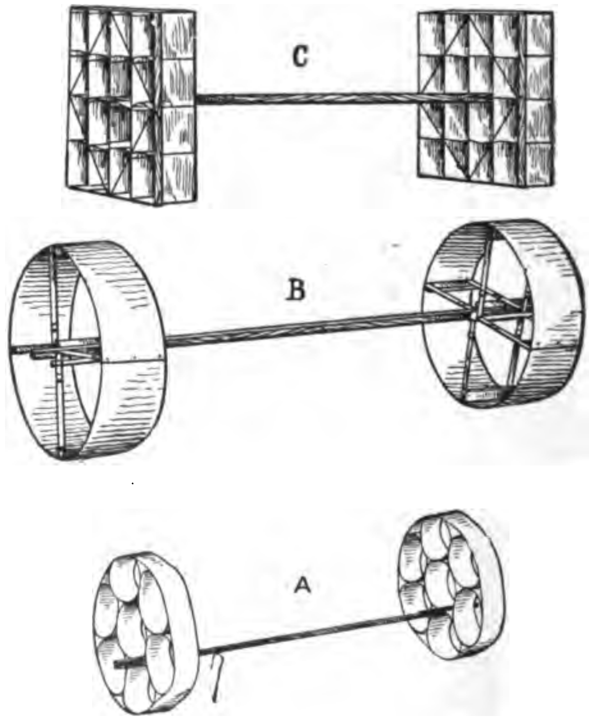


PLATE III.

by clock-work, but latterly by rubber bands, which proved more powerful for the same weight. The models flew with considerable swiftness for some yards, and though they frequently broke in falling, their behavior while in the air enabled the experimenter to judge their merits and to see the way to further improvements.

Wishing to prolong the flights of his models, Mr. Hargrave next substituted compressed air for the rubber as a source of

Facing p. 290.

FIG. 1.

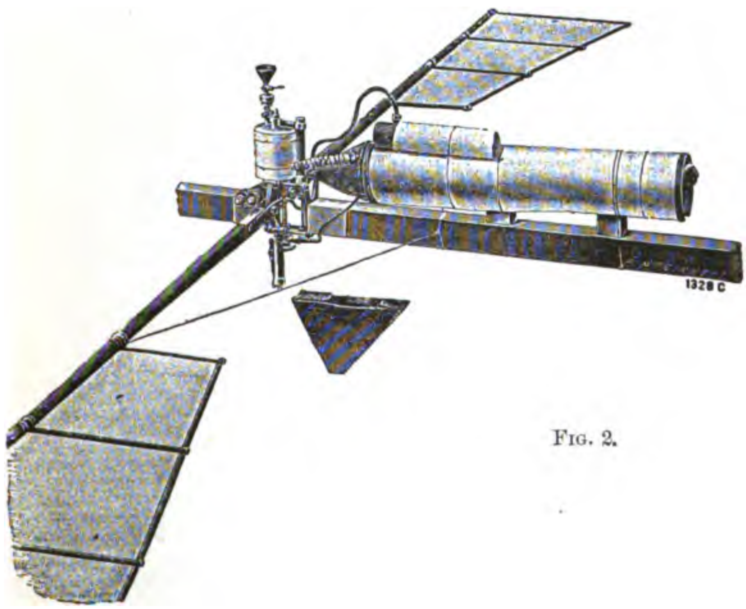


FIG. 2.

power. This gave rise to the type shown in fig. 1, which is the outgrowth of a great many constructions and trials.

It will be observed that the receiver, which is an elongated tin tube, serves as the backbone of the machine, upon which is mounted the body plane and the propelling mechanism. The slender rod projecting from the front is a safety stick, which is supposed to bend or break, thus lightening the fall of the model as it plunges to earth at the end of its flight.

The engine is a marvel of simplicity and lightness. Its cylinder is made like a common tin can. The cylinder covers are cut from sheet tin and pressed to shape in a vise. The piston and junk rings are made of vulcanite, and the cup leather packing does away with the necessity for the cylinder being either round or parallel. Though the efficiency of the motor was but 29 per cent. the model is reported to have flown 343 ft.

Encouraged by the success of these experiments, Mr. Hargrave in 1891 proposed to construct a steam motor which should equal in lightness and power the best compressed air motors thus far constructed, and which should supply a more even pressure and operate for a longer time. Such a motor he exhibited before the Royal Society August 3d, 1893. The engine was patterned after those of the compressed air type, but better constructed. The boiler was made of 12 ft. of ¼-in. copper tubing in the form of a double-stranded coil covered with asbestos cord and placed just over the backbone of the machine. It was heated by methylated spirits of wine drawn from a tank above, vaporized and spurted into the coils. The total weight of the flying model was 64.5 oz., including 12¾ oz. for the strut and body plane and 5 oz. for spirit and water. The power developed was 0.169 H.P., giving a speed of 2.85 double vibrations per second. It has been estimated that if 10 oz. more of spirit and water were added to the motor the whole would weigh as much as one of the compressed air models and fly 1,640 yards, or nearly one mile.

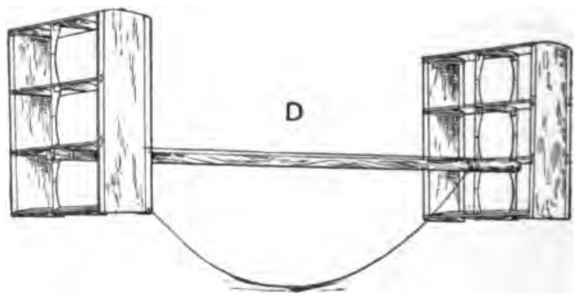
A much greater distance might be covered by the motors described in Mr. Hargrave's present paper; and even these, he tells us, can be relatively much lightened. He has found that the weight ratio diminishes as the motors are enlarged; as this cannot go on indefinitely, it will be interesting to learn what may be the probable limit of such improvement.

The kites described in his paper represent an effort to design a body plane of greater lift and stability. They are tested either by flying them as ordinary tailless kites or by projecting them from a cross-bow and noticing their behavior as they glide forward through the air. They remind one of the compound type of aeroplane adopted by Mr. Phillips, of England, a type which is especially worthy of study because it would seem to lend itself to rigid mechanical construction. It is to be hoped that Mr. Hargrave will find this last research a fruitful one.

Engine No. 18 (Plate I.) is the second steam motor for a flying machine made by the writer. Its total weight, without

spirits, water or body plane, is 5 lbs. 11 oz. This weight includes 6 ft. 9 in. of 1¼-in. × ¼-in. red wood, forming the strut for the body plane.

Eleven different burners have been tried. The most reliable arrangement is to put all the spirit on at once. The flame is steadier than that of No. 17, in consequence of the spirit being heated by its own flame before it has passed between some of



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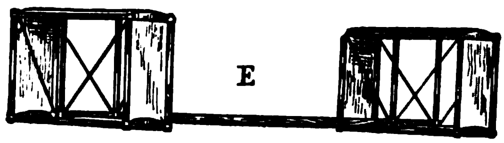

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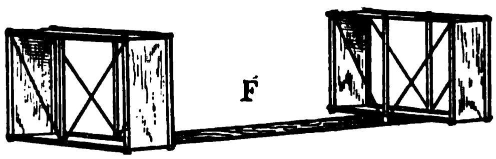

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PLATE IV.

the turns of the water boiler. The flame striking the water boiler first has a tendency to vary the supply of heat to the spirit holder.

Some of the first burners were supplied with spirit by a feed pump. Four boilers have been made: The one in the phototype has three coils, 1.6 in., 2.6 in. and 3.6 in. respective diam-

eters; made of 21 ft. of copper pipe .25 in. external and .18 in. internal diameters, weighing 87 oz. It is now known that a coil of equal capacity can be made weighing only 8 oz. and still be excessively strong.

The cylinder of No. 18 is 2 in. in diameter, and the stroke is 2.52 in. The feed pump ram is .266 in. in diameter. The piston valves are .8 in. in diameter. The $\frac{1}{16}$-in. steel link-work and brackets on the cylinder bottom are too light and will not stand the rough usage required of them.

The wings are 14 in. from the fulcrum to the inner edge of the paper surface. The paper is 22 in. long, 4 in. wide at the inner end, and 9 in. wide at the tip. The dimensions are the same as in No. 17, with the addition of 32½ sq. in. area at the tip of each wing.

On one occasion this motor evaporated 14.7 cub. in. of water with 4.18 cub. in. of spirit in 40 seconds. During a portion of the time it was working at a speed of 171 double vibrations per minute.

The diagram shows a net mean pressure of 95.6 lbs. per square inch, which makes the maximum indicated H.P. .653. Could this speed be relied on continuously for a few minutes the comparison between Nos. 17 and 18 would stand thus:

No. 17, with 5 oz. of fuel and water, indicates .169 H.P., and weighs 3½ lbs.: No. 18, with 21 oz. of fuel and water, indicates .653 H.P. and weighs 7 lbs.—that is, roughly, the weight of motor has been doubled and the power increased fourfold.

Comparing the area of wings, the speed of the engines and the wing arcs being nearly the same, the extra .48 H.P. is absorbed in driving the 65 sq. in. of extra surface at the tips of the wings.

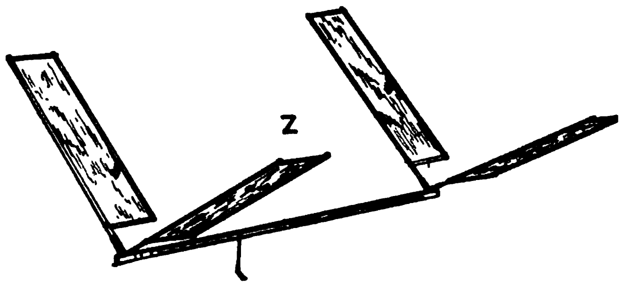
The following tabular statement of some of the results of No. 18 will be a guide to any one experimenting with engines of this size and quality of workmanship:

Date of Trial, 1892.	Spirits burned, cubic inches.	Water pumped, cubic inches.	Double vibrations.	Time, seconds.
November 25.....	4.18	14.7	74	40
December 5......	8.08	15.57	104	80
" 7......	8.46	23.78	248	240
" 7......	8.46	19 0	152	116
" 10.....	8.46	20.76	160	140
" 14.....	8.46	10.81	100	70
" 14.....	8.08	18.84	85	63
" 16.....	3 46	30.27	206	152
" 16.....	8.46	12.9	100	56

Some thrust diagrams were taken from No. 18, showing that 1.75 double vibrations per second produced a thrust of 1 lb. It takes 2.86 double vibrations per second of No. 17 to produce 1 lb. of thrust.

Three steam two-bladed screw motors (Plate II.) were made, the screw arms being hollow, with steam jet holes 5.5 in. from the boss; the jets reacting at right angles to the direction the blades were moving in. When the water was pumped into the boiler by hand a thrust of over half a pound was obtained, but the bearing soon got hot and stuck. When a feed pump was attached driven by an eccentric on the boss of the screw, the speed of revolution was reduced so much that the motor was practically ineffective, besides which there was great difficulty in getting a very small pump to work at a high speed.

Before beginning another motor it was thought advisable to try whether a better disposition of the supporting surface—or body plane, as the writer terms it—could not be found out, and at the same time to see if any foundation could be discovered for the assertion that birds utilize the wind in soaring. No amount of observation of birds will solve the soaring problem; it can alone be done by making some form of apparatus that will advance against the wind without losing its elevation.



The expense of constructing and erecting a large whirling machine similar to Professor Langley's or Mr. H. S. Maxim's being too great, and knowledge of the fact that planes or other things moving at the end of an arm through still air are not under the same conditions as bodies flying in disturbed air determined the selection of kites as the best means to the desired end.

Plates III. and IV. are some of the kites, and are sufficient to indicate the extent of the field now open for experiment. The novelty, if any, consists in the combination of two well-known facts:

1. That the necessary surface for supporting heavy weights may be composed of parallel strips superposed, with an interval between them. (Described by Wenham in 1866 and adopted by Stringfellow in 1868. The writer made an experiment in 1889 with superposed planes, but failed to show that any additional support was obtained. Professor Langley showed by inference that there is an additional support. Pages 33 and 47 of "Experiments in Aerodynamics, 1891.")

PARTICULARS OF KITES.

NAME.	Number of cells in each section.	Length of each cell parallel to the connecting stick.	Breadth of each cell horizontally at right angles to the stick.	Height of each cell vertically at right angles to the stick.	Radius of curved horizontal surface.	Length of the stick between the sections.	Material of which the surfaces are made.	The point of attachment of the string is distant from the forward section.	Weight of kite.
A......	7	2 in.	3.75 in.*	3.75 in.*	...	24 in.	Paper.	4 in.	2.5 oz.
B......	1	4.5 "	13 " †	13 " †	...	30 "	Aluminium.	11 "	14.75 "
C......	16	3 "	3 "	3 "	...	22 "	Cardboard.	6.5 "	10.5 "
D......	3	4 "	13.125 "	4 "	4.5 in.	31.625 "	Wood and paper.	12 "	11 "
E......	1	4 "	10.7 "	6.25 "	4.5 "	21.25 "	" " "	7.25 "	3.25 "
F......	1	4 "	10.7 "	6.25 "	...	21.25 "	" " "	7.25 "	3.25 "

* Distorted cylinder. † Cylindrical.

2. That two planes separated by an interval in the direction of motion are more stable than when conjoined. (Patented by Daujard in 1871. Made and exhibited by D. S. Brown in 1874.)

The form which the complete kite assumes is like two pieces of honeycomb put on the ends of a stick, the stick being parallel to the axes of the cells. The cells may be of any section or number; the rectangular cells are easiest to make, and if the stick or strut between the two sets is placed centrally, as in kites *B* and *C*, it is immaterial which side is up. Practically the top or bottom is determined by imperfections in the construction. This is of particular advantage for flying machines driven by a single screw The rectangular form of cell is also collapsible when one diagonal tie is disconnected.

These kites have a fine angle of incidence, so that they correspond with the flying machines they are meant to represent, and differ from the kites of our youth which we recollect floating at an angle of about 45°, in which position the lift and drift are about equal. The fine angle makes the lift largely exceed the drift, and brings the kite so that the upper part of the string is nearly vertical.

Theoretically, if the kite is perfect in construction and the wind steady, the string could be attached infinitely near the center of the stick, and the kite would fly very near the zenith.

It is obvious that any number of kites may be strung together on the same line, and that there is no limit to the weight that may be buoyed up in a breeze by means of light and handy tackle. The next step is clear enough, namely, that a flying machine with acres of surface can be safely got under way or anchored and hauled to the ground by means of the string of kites.

If the string of kites gets into contrary currents of air, kites and suspended weight may be disconnected from the earth, and will remain supported, drifting in a resultant direction, determined by the force of each current and the number of kites exposed to it.

Kites *E* and *F* are of equal weight and area. In *E* the horizontal surfaces are curved, with the convex sides up. *F* has all the surfaces plane. Roughly, *E* pulls twice as hard on the string as *F* does. So that a flying machine with curved surfaces would be better than one with a flat body plane if the form could be made with the same weight of material.

This is proved in another way. The old windmill shown in the paper last year was fitted with four flat sails, which could be changed for four curved ones. When the flat sails are turned so that they and the axis are in two planes no rotation takes place. But when the curved sails are put on symmetrically with the chords of the curves and the axis in two planes, there is a slow and powerful rotation in the direction of the convex sides of the sails. Rotation ceases when the sails are twisted in their sockets so that the wind is tangential to the curve of the sails about three-quarters of their width from the forward edge.

There is no doubt that the wind drawing into and striking the concave side of the sail is more powerful than the current impinging direct on to the forward part of the convex side, although the hollow surface is altogether masked by the rounded surface.

Both the kite and the windmill experiments refer to moving air passing stationary bodies. When the kites *E* and *F* are discharged from the cross-bow in calm air they both have the same trajectory.

As to the solution of the soaring problem the only fact observed is, that on a gusty day kites *E* and *F* both shoot up nearly overhead and slack the string into a deep bight, then drift away to leeward until the string brings them up again. This wants careful and undisturbed observation. The writer unfortunately had to experiment in public. It is clear that the wind must be considered as volumes of air of different densities.

Kite *Z* has four flat planes 4 in. × 15 in. The angle between each pair of planes is 108°. A similar one with curved sails was difficult to adjust. Both flew fairly well, but they cannot be compared with the cellular form for steadiness ; and it is certain that the numerous accidents that have happened to the india-rubber and compressed-air driven machines have been solely due to imperfections in the flat or V-shaped body planes.

SUGGESTIONS AND EXPERIMENTS FOR THE CONSTRUCTION OF AERIAL MACHINES.

By F. H. Wenham.

Twenty years have elapsed since the author directed attention to some investigations he had conducted relating to the conditions of aerial transit and the flight of birds.

The experiments and deductions were specified in a paper read at the first meeting of the Aeronautical Society of Great Britain, under the title "On Aerial Locomotion and the Laws by which Heavy Bodies Impelled Through Air are Sustained." For many years after that essay was published the pressure of other occupations prevented the writer from personally continuing or conducting further experiments. The inference from those already tried had tended to show that flight for man could not possibly be effected if attempted with any arrangement designed in imitation of the beating wings of a bird, either in their form or mode of attachment, for the reason that we should not be able to obtain strength of construction combined with the requisite lightness by the application of any independent motive power and its consequent weight.

But it was also shown that by other modes of arranging the supporting surfaces the achievement of flight was apparently quite feasible.

The fact was demonstrated that the supporting effect of inclined planes or surfaces in rapid horizontal motion in air, applied for the purpose of counteracting the force of gravity, depended not upon the superficial area or size of the surface, but on the condition of its lateral extension transversely to the direction of motion, for the reason that effective sustaining force can only be obtained with maximum advantage by the *first impact* on a very wide strata of air, exemplified in the long, narrow wings of some birds, such as the albatross, which may be said to reside in the atmosphere, as it does not rest on the earth or water for days at a time.

To effect this length of wing in a compact compass for the construction of a flying-machine, illustrations were given in which the aeroplanes or supports were superposed in a series resembling an open Venetian blind. Each separate stratum of air was found to be thus as effective for flight, when arranged in a system of short detachments, as if widely stretched out and utilized by one entire length of wing.

On reviewing the very numerous suggestions and actual constructions of flying-machines, it is remarkable how these elementary conditions have been ignored or not even comprehended. In many of the subsequent wing arrangements for flying-machines the very opposite form has been adapted, with the supporting surface extended back lengthways, having the narrow end in front—a very hopeless form for the purpose in view.

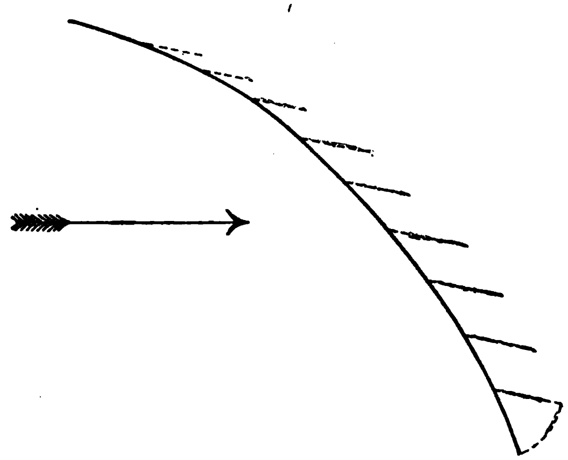
Generally, for the sake of illustration, the supporting surfaces have been spoken of as "planes," but for a suitable application in an elastic yielding medium like air, the form expressed by this term is not strictly correct. The surfaces should have a form with a progressively increasing incline, known as an "expanding pitch," as adopted in the best form of screw propellers. This requisite increase of pitch for elastic air is far more rapid than for inelastic water.

If we take a strip of fabric having some weight of material —preferably with a smooth surface—such as oilcloth, and trail it against a current of air, it will extend outward in nearly a parabolic form, thus :*

The stratum of air caught by the top edge becomes immediately deflected downward, and deviates in a different direction, by which the underlying position is influenced and follows in like course, giving a special form of curve to the fabric. We have here but little supporting effect, this being principally confined to the top edge. The after part of even a curved surface of this description is quite ineffectual for sustaining weight. The pull or forward resistance caused by its extension is very great. The curve in its continuation to its lower end takes the direction of the deflected current of air, or nearly so. But suppose we arrange that it shall not do so, but, instead of a curve, stretch out and constrain the surface to follow in a straight line or plane, directed from the angle or leading portion of the front edge most effective for sustaining weight, would the terminal part of the flat surface be effective

* See fig., p. 299.

for a like purpose ? Quite the reverse. It would actually be *dragged down* by the currents of air deflected from the first part or edge, so that if this were to be set *per se* at the best incline for support and minimum resistance, the descending air or following portion would compel the front edge to take a more obtuse angle, improper for the least amount of forward resistance. In order to meet the varying directions of incompatible deflected currents, the whole surface has to take an average inclination exceeding the lowest degree at which a narrower surface (having in consequence a diminished resistance) would require to be set.



If such a form is given to the following portion of the parabolic surface as to derive some abutment against the receding air that has been previously deflected from a leading portion, the final part is but of little use for sustaining any weight, because it is wastefully *acting against a yielding support on air that has before been carried away from it and so used up.* After the first impact, and the air begins to recede, the sooner that it is got rid of the better. Let all weight be sustained by *first impact* as far as practicable.

Referring again to this parabolic or wind curve : Supposing that we copy the figure and give the same outline to a thin sheet of metal fixed in like form and position. This has not altered the conditions. The effect as regards the action of the air remains similar for the same velocity of currents ; then let the metal be cut through in numerous equidistant strips, and all of them turned back at the same angle from the horizontal as shown by the dotted lines at the back of the curve. We have thus cut up and destroyed the surface so that the air

rushes through it in parallel strata. Instead of loss of effect, the *gain* under these circumstances is remarkable as merely arising from this apparent disunion of surface into fragmentary portions. The bulky body of air that was before turned down so as to be useless near to its final exit now passes through in straight lines with very little deflection, each stratum giving up the force of its first impact and maximum lifting effect before it eventually leaves the interstices of the *louvres*.

The tangent turbine is one of the most effective forms of water motor ; it consists of an extended rotating ring of thin, narrow blades, somewhat hollow or curved in cross outline, which rotate round a current of water radiating from the axis of the machine. The impact of the first portion only of the water on the narrow blades is taken as serviceable ; the residual force of the spent or "dead water" is not worth consideration.

When a narrow blade inclined upward is propelled at a high speed through air there is no doubt a diminution of pressure on the top surfaces, which tends to increase the lifting effect ; all thickness of substance, however placed, given on the upper side of the plane, will diminish this ascensional force. The front edge of the wing of a bird is thick, as a matter of necessity in natural construction, and in no way aids the faculty of flight. Among other experiments by the writer, a system of aeroplanes was made out of the thinnest tin plate that could be procured—.005 in. thick and weighing 8 oz. to the foot. The blades were concave beneath. The lifting power of this arrangement exceeded anything before tried, and conveyed the idea that the best effort would be obtained if the supporting surfaces were to be made of smooth metal. Strips of aluminium might be used, for the sake of lightness ; the proper surface curve to be given by passing them lengthwise through rollers turned to a similar curve.

Experiment has proved the lifting power of extended planes superposed or situated one above the other at intervals of only a few inches, but these must first be utilized with the quick starting velocity necessary for support and transit through air. Under these conditions how could a safe descent be effected ? For it is apparent that to avoid risk we must then slacken speed. There would not be sufficient spread of surface to act as a parachute, and some contrivance resembling this would require to be quickly unfurled for the purpose. Even if this action could be obtained, its application would greatly increase the difficulty of construction, and its bulk when furled would prevent the machine from cutting the air with a knife-like absence of forward resistance. The supporting surfaces, if packed directly one above the other, cannot offer a sufficiently safe support for a nearly vertical descent, if this should be requisite in case of mishap or other circumstances. But it does not follow that the aeroplanes need be placed vertically. They will be equally effective if carried back in open steps. As so arranged, the collective surfaces will increase the horizontal area or wing expanse necessary for a safe descent.

The question arises whether the upper or the lower lengths

should be taken in advance. Several reasons might be adduced in favor of the former arrangement, and why the top stratum of air should be first utilized, for this would cause the equilibrium to be more definite and certain.

We are as yet only in the region of suggestion. Many of these questions might be settled by kite experiments wherein we have an independent motive power by which the air is brought against us, instead of the machine being propelled against the air, which action is precisely the same in all its conditions and mechanical effects relative to the progress through it.

The application of any motive power adds enormously to the difficulties attendant upon success. Many of the inventors of flying arrangements have been directing their attention mostly to the question of light motive power, with but a vague notion of the form of aerial machines to which they are to be applied. A series of kite experiments would go far to determine these conditions before a free flight by descent was decided upon.

The writer tried some experiments with kites, but made no record thereof, as nothing particularly worth noting occurred. The most remarkable experiment was as follows: A kite was made in the form of a complete circle with a rim of split cane, and tissue paper stretched and pasted thereto. This flew much as other kites will do, but was rather unsteady, necessitating the addition of a short tail to guide it. The circle was 18 in. in diameter.

The same kite then had the center of the paper cut out to a circular opening of 13 in. in diameter, thus reducing the former area to just one half. The edge of the opening was strengthened by pasting tape around it. On flying the kite again, though it looked as a mere circular ring, the flight was almost perfect for steadiness. It rose and floated equally well as with the former double area of surface, the absence of which did not appear to diminish the raising power.

The explanation of this is that a fresh understratum of air, undisturbed by the advancing edge of the ring, acted independently upon the following inner edge, and that this more than compensated for the loss of half the area occupied in the former experiment.

Very little has been effected in the way of experiment with dirigible parachutes. The earliest record of this was at the commencement of the present century. Eliza Garnerin, daughter of the aeronaut, was the first female who ventured to quit the balloon in the frail parachute, and afterward performed the perilous experiment no less than thirty-nine times.*

* "The parachute, which seems to have no other aim than to moderate the shock in falling—the parachute even has been found capable of being directed: and aeronauts who have practiced it take care not to forget it. If the current is about to drive the aeronaut over a place where the descent is dangerous—say a river, a town, or a forest—the aeronaut perceiving to his right, let us suppose, a piece of ground suitable for his purpose, pulls at the cords which surround the right side, and by thus imparting an obliquity to his roof of silk, glides through the air, which it cleaves obliquely, toward

In practical flight the concensus of invention seems to have been in the direction of inclined planes or surfaces driven by a screw. Now, to economize power, the form of every cross-section of the blade of the screw is also a most important consideration. The best form of screw blades working in water, for marine propulsion, should have what is termed an "expanding pitch"—that is, the angle should increase progressively, from the leading to the following edge, in a ratio determined by the known "slip" or yield of the water in crossing the blade. Further, as in common with all rotating disks, a slight centrifugal action takes place. This should be counteracted and economized by converting it into a direct thrust by giving the blades what has been termed a "conchoidal" form—that is, instead of each radial section of the surface of the blade taking a right line from the axis, it should be curved forward on the propelling face, the curvature increasing toward the circumference. If this has been found to be an advantage in water—which is quite inelastic—how much more would these requirements have to be considered for a screw propeller in elastic air, wherein the yield is comparatively enormous and the centrifugal action much greater on account of the high velocity of rotation required. At present the writer merely calls attention to these conditions, without venturing to give any formula. The data for any precise form can only be arrived at by experiment. All the so-termed screws which he has seen as applied to flying models and machines have been mere flat plates fixed on to radial arms.

These remarks on the screw propeller are offered in the conviction that form in both the supporting surfaces and also in those for propulsion is of the utmost importance in aerial support and progression.

From the inferences arrived at by direct experiment the author considers that flight can only be achieved on the principle of *first impact* obtained by means of stratified narrow blades variously placed; yet from the action of the different models tried the conviction has been that, if the sustainers or planes we e to be merely superposed vertically, as in a Venetian blind, the machine would fail to act in free flight, for this reason: a violent fore and aft oscillation would occur, constantly altering the angle at which the planes were set, causing the arrangement to trip backward The remedy for this would be to extend the base line and arrange another and smaller system of superplanes set some distance aft, at the end of the base rod or foundation. These two positions of support would probably insure steadiness of flight.

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the desired spot. Every descent is, in fact, determined by the side on which the inclination is greatest. . . .

"Mlle. Garnerin once wagered to guide herself with a parachute from the point of its fall to a place determined and remote. By the combined inclinations which could be given to her parachute, she was seen, in fact, very distinctly to manœuvre and turn toward the place designated, and her wager was gained almost within a few yards."—"Astra Castra Experiments and Adventures in the Atmosphere," by Hatton Turner, Rifle Brigade, 1865, pages 342-43.

DISCUSSION BY PROFESSOR TODD, OF AMHERST COLLEGE.

Mr. Wenham's suggestions appear to me of the utmost significance. The great length of the supporting plane, in proportion to its width, and the device of *louvres* in steps retreating upward, are forms with which I have independently experimented during the past year and a half, and I can testify to their correctness in principle. Herein lies the vast advantage of the machine ultimately over the bird. The individual *louvres* need to be curved backward, and it is not harmful to diminish the width symmetrically toward both ends. The importance of smooth surfaces cannot be too prominently kept in view. In some of the most successful experiments I have made with models, strips of thin glass plate formed the aeroplanes. The greater weight was not objectionable, and the loss of the plane with each trial was a matter of no moment; a new one could be put in place quite as quickly as a deformed metallic one could be bent back into shape. In building a machine to be driven through the air by a screw, and supported by the action of aeroplanes, the physical conditions which secure the maximum efficiency of these mechanical agents must not be forgotten. The theory of the aeroplane requires that it shall move constantly upon fresh and undisturbed strata of air, while the theory of the screw propeller demands that its advance shall be continually into a like medium. Obviously then there is little choice between devices which have the screw directly ahead or astern of the aeroplane system; either construction is so faulty that failure is much more likely than success. If, however, the screw propeller is placed centrally and has abundant leeway, while the aeroplane system is bisected and disposed symmetrically, a few of the conditions essential to success would seem to be met, although the problem of stability would remain by no means an easy one. This appears to me best secured by curving the aeroplanes upward on either side and attaching them as high as possible to the rest of the mechanism—higher, for example, than the relative points of attachment of the wings of a bird.

SUGGESTIONS AS TO METHODS OF EXPERIMENTATION.

BY A. P. BARNETT.

THE problem of mechanical flight (and by this I mean moving *through* the air and not *with* it, as in soaring) could be more easily solved if an easy and progressive method of experi-

mentation were adopted. It is certainly unwise to attempt to fly a machine from some high point downward before the machine is proven capable of rising from a low point, because a first attempt almost invariably results in partial failure and the machine is liable to be broken. Even if we had a machine fulfilling perfectly the conditions necessary to flight, the art of successfully controlling it could not be acquired immediately nor intuitively; it would have to be acquired by progressive steps.

Inasmuch as flight is to be a rapid projection, probably the best way to rise and to alight would be to acquire an initial velocity upon the earth, leaving it and striking it at a slight angle. Screws or wings lifting vertically, unless the machine be in forward motion, soon agitate the air so that their lifting power is not so great as if the machine were moving forward into new and undisturbed air.

For an experimental machine, I would suggest that the frame of the car, containing the motors, fuel, and operators be made of suitable rods drawn together at their ends to a common point and separated at their middle by proper bracing, and that it be left unenclosed, thus allowing the operators a full and unobstructed view. This car frame could be mounted upon three pneumatic-tire bicycle wheels, two forward fitted with brakes, and one rearward moving freely like a caster, so that the machine could be easily guided to the right or left, which should be done with the rudder. The machine in this condition would be ready for the first class of experiments, which would be to test its capacity to acquire high speed, and to learn the use of the rudder. In order that ground friction should be reduced to a minimum, a smooth level platform or floor of suitable length and width should be laid, around which should be a level stretch of ground. Perhaps some suburban street paved with asphalt would serve instead of a floor.

Those who are used to riding a bicycle know how easy it is to propel one's self over an asphalt pavement as compared with a rough block pavement. The first trial as to speed should be well within the capacity of the machine, and at each succeeding trial the speed could be increased until the maximum is reached. In order to give the machine the same air resistance that it would have if the aeroplane were attached, a small plane could be set vertically which would have the same resistance as the aeroplanes were calculated to have. Speed tests should again be made, and there should be no attempt to rise until good results were obtained, as a slow speed would require aeroplanes so large that they would be very difficult to brace or to control.

But if it is found that high speed can be attained, the aeroplanes are to be attached. Various kinds of planes could be tried. I am in favor of two or possibly three sets of superposed planes set at a diedral angle. The plane of the tail should always be kept at a more or less obtuse angle with the longitudinal axis of the machine or car, so as to prevent it from pitching head downward.

With the planes attached the same tentative method should be pursued. The machine could be run at an increased speed for each experiment, so that when the first flight was attained it would be but a very short distance, and thus no damage would be done. The length of flight could then be increased at each trial until the length of the platform was reached, except sufficient in which to stop after alighting. As soon as the wheels touch the ground the screws could be reversed and the brakes applied and the machine stopped very quickly without any jar. But if it were found that flight could not be accomplished with the maximum speed of translation, lifting screws could then be attached in addition to the aeroplanes, and the same method of experimentation resumed.

By this careful and tentative procedure the art of controlling the machine in flight could be acquired, step by step, without serious accident. Each difficulty could be met in its proper order and faulty construction thus more certainly located. Hundreds of experiments could be made without accident; whereas if a machine were built complete before any experiments were made, even though successfully accomplishing flight, it would most probably be seriously damaged in the act of alighting, thus causing vexatious delay and expense.

I have assumed that it is no longer a question whether sufficiently light and powerful motors can be built, and I understand that assumption is well taken, based upon results already obtained.

DISCUSSION BY O. CHANUTE.

The method of experimentation proposed by Mr. Barnett is sagacious, prudent, and practically the same which has already been employed by Mr. Maxim, except that the latter has built a railway track instead of a "smooth level platform," and thus has largely diminished the ground friction.

Mr. Moy ascertained in 1875 that he could not gain sufficient velocity to rise, in running over a platform with his machine, although his steam-engine weighed only 27 lbs. to the horse power.

Now that Mr. Maxim has an engine weighing only 8 lbs. per horse power, and a railway track to diminish the ground friction, he can undoubtedly rise upon the air, and the next question is whether his machine will then preserve its equipoise.

It would seem, therefore, that the important problems now to solve are those of equilibrium and of alighting safely, and to these I would particularly draw the attention of those investigators who are interested in the subject of dynamic flight.

Simple and cheap experiments, made with models dropped from a height or projected, by a crossbow, such as that used by Mr. Hargrave, may throw considerable light upon the solution of these important problems.

LEARNING HOW TO FLY.

By C. E. Duryea.

The object of this paper is not to present anything new, but rather to call to attention and to impress upon the mind as deeply and fully as possible a few facts which constitute the key to complete success at flying. Without this key our efforts, time and money will be wasted; with it success is ours. There need be no longer a question as to our having within our reach all the necessary mechanical features required for flight. We can build machines light enough, equip them with motors powerful enough, and can safely say, "They will fly if properly directed." Right there, however, is the problem. "If properly directed" is an unknown quantity. Solve this and we will fly as soon as the machines and motors can be built. This many admit; but I fear that even they do not fully see the direction from which we must expect help. I fear that they are spending time needlessly in attempts to improve the motor or the machine, or in seeking for some automatic method of managing instead of going directly at work to fly.

The proper way to solve this question is to build a machine and to use it. Use and use alone can fully show its good and bad features, and point out the way to properly remedy them. Practice leads toward perfection; so, if we expect to do anything properly, we must practice. We learn to walk, talk, write, ride, swim, skate—in short, everything requiring skill is the result of practice; and flying will be no exception to the rule. Suppose for an instant that we had the most perfect flying-machine of the next century right here before us. Could we use it? Would we have the practice, skill and increased knowledge necessary to properly manage it? No. It would be as useless to us as our crudest experiment of to-day. We would succeed in breaking it, and, in all probability, our necks with it, at the first trial. But if we had such a machine, what would be our first duty toward it? Simply to learn to use it. This, then, is our duty toward the proposed machine of to-day, for who knows that it is not as successful a form as we will find? All around us we see pursuits requiring various degrees of skill to properly follow them; and among them is one—cycle-riding—which calls for a skill very similar to that required for flying. The cycler must continually guide and continually balance. Is it difficult? No. Simply because continued practice has given a skill that overcomes the difficulty with ease. Take away one of the wheels, and yet a fancy rider will guide, drive and balance on the other. What can be more unstable than that which rests on a single point? The expert is able, however, to ride the single wheel where he chooses, even up or down a flight of steps. Practice and practice alone enables him to do this. I cannot believe that the

flying-machine will prove so difficult as the single wheel, nor do I see any reason why it should be more difficult than the two-wheeled vehicle now so common. Let us compare it with this. Suppose one of our most modern cycles to be presented to a ruler in Central Africa with the information that it could be ridden and would be found faster than his fastest horses. Suppose that he called together his best men and asked them to ride it under penalty of instant death if they fell off or in any way failed after attempting to ride it. How many do you suppose would make an attempt? Or, having attempted, would succeed? None. The machine would not be ridden. So it is with our attempts at flying. Those who have attempted have failed with disaster to themselves and machine before they had acquired skill enough to properly manage their creation. This should not, need not be. We do not teach a man to swim by throwing him into deep water and telling him to swim or drown. Instead, we support him with a rope from the end of a pole till he is able to go it alone, till he has acquired confidence and skill. Likewise with the cycle. The pupil is held in position by the instructor till he balances and steers almost automatically. He is not allowed a chance to fall until he is thoroughly familiar with every movement; and instead of a nervous beginner, straining every muscle in an effort to do with strength alone the work before him, we have a full-fledged rider who sits easily, rides easily, and accomplishes more than the beginner possibly could, simply and solely because he has the skill. So it will be with the flying machine. We contemplate powerful motors as necessary to drive us through the air at a rate that will cause the air to support us, while the gull and the buzzard, by their skill, take advantage of the air currents and soar for hours without any appreciable work at all. It is the skill rather than the motor that we need. Can we get that skill? Certainly, by practice, just as the swimmer and rider get it. This is the one thing yet lacking, and to this we should direct our attention. "But," you ask, "how shall we practice long enough to get this skill if our first attempt ends in disaster and we are unable to make a second?" The answer is simple. Just as the rider and swimmer go to a school for their instruction, so we must go to a flying school for ours. The first swimmer avoided deep water until he had learned enough to venture safely; the first rider had to take his falls as best he could; but in these respects they had the advantage over the would-be flyer. If it were not so, in all probability we would have had no swimmers or riders. The flyer must make his first attempt in mid air, and as this is suicidal without protection of some kind, he must be provided with a flying school. In other words, the key to flying is the school. "And what is the school?" you ask. It is any one of several arrangements for supporting the flyer in mid air, safe from danger but free to practice. There is nothing new in the idea, and I claim no credit for it; but I do earnestly desire to impress upon my readers the fact that we will never learn to fly without the school, and that we may

fly at once with it. Sanderval stretched a cable between two peaks and suspended his apparatus from it; others have used derricks and beams. These devices are good as far as the idea goes, but they are far too limited. The birds do not fly in small circles; and we, with our proposed large machine and high rate of speed, must soar in still larger circles. I would suggest the use of a captive balloon, held by several widely divergent guy ropes, and preferably over a body of water, so that there would be less gusts to the air currents, and so that a possible fall would not be so risky. From this balloon, by a single rope, suspend the machine and rider so that they would be as free to swing as a plumb-bob. If his machine is provided with a propeller and a means of driving it, the experimenter can practice on "circles" and "figure eights" until he is thoroughly familiar with every detail of the machine and until he manages it almost instinctively. If he finds defects in the design or construction they are not even dangerous, but can be remedied at once and the result noted. A spring scale in the suspending rope would show at a glance what he is doing toward sustaining himself, and whether any change improves the result or not. There is no guesswork about it, but a positive progress can be relied upon. A different rider may try his ability as fast as the one practicing gets tired, and notes and experiences can be compared. Improve the machine as seems best and continue the experiments with those riders who are most apt, and a short while will find them going it alone. This device is superior to others in many respects. It gives the beginner ample room to swing around in, and the room is an absolute necessity for the flying-machine learner. He must move at a good speed if he is to fly at all, and he requires time to think how to act until action becomes a sort of second nature. The atmospheric conditions are better up high, because we thus avoid the gusts and eddies at the surface. Any kite-flyer appreciates this difficulty. If the machine is provided with a motor and operated only on quiet days this disturbance would not be so appreciable; but as absolutely still days are few and far between, it is not wise to ignore it. If an aeroplane patterned after the suggestions of Le Bris, Mouillard, Lilienthal and others is used, it is almost absolutely necessary to have the aeroplane above the gusts and eddies, for, having no motor, it cannot help itself through them as the bird does by flapping; and any cycle instructor can tell you how greatly the effort of teaching is increased if the surface of the practicing yard is in poor shape. Bad conditions will be much more detrimental in the air, because the rider can see the rough places in the ground, but the flyer cannot. When these things are taken into consideration it is really a matter of surprise to me that the experimenters mentioned were as successful as they are reported to have been. The birds themselves acknowledge this difficulty by flapping oftener when near the ground. How essential, then, that we avoid this unfavorable region in our lessons; and the most available, if not the only way to do so, is by using the captive balloon. With it we can

soon get the skill which will enable us to handle with success almost anything that possesses the elements of a flying-machine, just as an expert cycler will ride anything that looks like a cycle and some things that don't.

It is not the purpose of this paper to advocate any special form of machine or to go into its details; but I would suggest that a machine, bird-like in form, provided with a vertical and horizontal rudder controlled from a handle-bar, and having a propeller to be driven by the feet of the rider, contains all that is required for a short flight; and if we should be able to take advantage of the air currents as the birds do it might be all we require for longer ones. Such a machine would be simple, light, not likely to get out of order, and would cost but a few hundred dollars. The captive balloon would cost probably $100 each ascension, which would give several persons one or more short lessons each. On this basis $1000 might suffice to secure actual flight. On the other hand, if many changes should be found necessary in the machine and a motor prove necessary, five or ten times that amount or even more might be required. It matters not, however, if twice $10,000 be required, the solution of the problem is worth many times that amount. Nor need money be difficult to raise. A shrewd advertiser paid $10,000 for a single Columbian half dollar. Surely more advertising would result from having their name connected with a series of flying experiments. Flying is within our grasp; we have naught to do but take it. And having once wrested from nature the secrets of the birds and achieved actual flight even with a small machine, capital will rush in and improvements will follow till in a short while the only universal roadway, the air around us, will be in universal use.

DISCUSSION BY PROFESSOR TODD, OF AMHERST COLLEGE.*

A friend suggests that criminals, in lieu of treatment by hanging or electrocution, be detailed for duty on flying-machines for the common cause of science and humanity. A man convicted of slaughtering his wife, for example, instead of being forced to edify a handful of curious onlookers with the ghastly spectacle of capital punishment, might be permitted first to receive the coaching of some expert in aerodromics; then, on the day set for public exhibition, if both machine and aviator go to smash, well and good—the criminal would have had to suffer death anyway, and the builder of the machine would feel compensated by the opportunity for testing his device; while if the trial succeeded, the gain to the art of flight may be enormous, and the culprit will come down presumably frightened enough to choose a life of virtue forever thereafter.

A PROGRAMME FOR SAFE EXPERIMENTING.

By L. P. Mouillard, Cairo, Egypt.

I have been asked to prepare a paper for the International Conference on Aerial Navigation, which is to be held in Chicago, and I feel that I cannot better promote the cause of aviation, and advance the securing of practical results by others, than by describing a method of experimenting for a soaring apparatus which I have long contemplated, and which I would most certainly have carried out myself before now if the failure of my health had not left me too crippled to perform the necessary manœuvres and exertions.

I have described this method at length in my second book, "Le Vol sans Battements" (Flight without Flapping), but as this will not be published in time for the Conference, I beg to offer to this assemblage the first fruits of my reflections during many years concerning the safest way of testing the merits of an aeroplane intended to soar upon the wind like a sailing bird.

The method is not new; it has been proposed many times, notably by Count d'Esterno and by Captain Le Bris, and it was apparently employed by Dante in his exploits over Lake Trasimene, for it simply consists in carrying on the experiments over a water-bed into which the aviator shall fall without injuring himself upon each mishap inseparable from first attempts; but some of the details may possess novelty and be of value to inventors who may wish to experiment with apparatus of their own designing.

The aeroplane to which I intended to apply this method of experimenting, and which I have described in "Vol sans Battements," is so designed as to admit of adjusting the wings to the speed of the wind, and of thrusting their tips forward or back of the center of gravity of the whole system so as to change the angle of incidence at which the machine meets the wind; this I consider a prerequisite of success, and I assume that the experimenter will have some equivalent arrangement.

The first condition of final success, in my opinion, is that the apparatus and its operator shall be entirely at liberty and free to glide in any direction. It has been proposed to experiment when suspended by a long rope, or when towed under a supporting track, or above a railway flat car with various restraining ropes to restrict motion. None of these will enable the apparatus to show what it will really do. It must be free to fall in order to teach the aviator how to handle it; and this freedom must be given just as soon as certain preliminary tests, which I will describe further on, have enabled him to ascertain the effect to be expected from his manœuvres.

The first question for an inventor who desires to test a soaring device is how he is to rise into the air—how he is to get a first start and bring his machine under control. Some imprudent aviators have proposed to launch themselves off from

great heights, or to cut themselves loose from balloons, which is little short of suicide; while others again have proposed to have themselves towed at high speed, or to descend an inclined plane until the air pressure due to the speed lifts their apparatus from the supporting car. None of these methods are compatible with safety until the handling of the machine has been thoroughly mastered.

Happily for aviators, I have stated it in "L'Empire de l'Air," and I now reaffirm it again, for it is the keynote to success in soaring flight: "Ascension can be effected by skillful utilization of the power of the wind, and no other force is required" ("L'Empire de l'Air," page 287). Indeed, man can, by dropping from a sufficiently great height, acquire such velocity by the fall that he will at once be in full flight, but he may soon thereafter come to grief.

If the wind be strong enough to carry the weight—that is, if it blows at a speed of 10 meters per second (22 miles per hour)—then, in order to enter into full flight, it is only necessary to so adjust the wings that they will correspond to this speed of 10 meters, and the course of flight will be level. If they are set at an angle to correspond with a less velocity, say 8 meters, the aviator will be slowly lifted; if set corresponding to 6 meters, ascent will be more rapid, and at angles corresponding to speeds of 2 meters or zero meters, or even less—that is to say, when the tips of the wings are carried far forward—ascent will inevitably follow.

This certainly is the most practical method of experimenting. It is necessary for success to have a wind of at least 10 meters' velocity (22 miles per hour), a sufficient surface not less than 10 square meters (130 sq. ft.) in area, and, finally, the courage to attempt the manœuvres. This plan requires, nevertheless, much less audacity than other methods of obtaining a start.

To succeed it will be necessary to have an aeroplane of considerable spread in relation to its width, at least in the proportion of six to one, which is almost the ratio of the vulture. It will be necessary to make the first experiments in the summer when the water is warm, for the first attempts to soar will surely result in a succession of duckings, and it will be best that they should not be disagreeable The experiment should occur over deep water; the body of the man and the aeroplane should both be so arranged as to surely float. He need not be able to swim, for the danger of drowning will thus be avoided. No experiment should be made except in a wind capable of raising the apparatus.

The manœuvre intended to be performed is to open the wings—that is to say, to carry the tips forward sufficiently so that the machine will be lifted. By this single displacement of the center of gravity ascent is produced.

There is no need of taking any risk in the first trial. We may begin little by little without losing the foothold. First, we rise a decimeter (4 in.), then, by ceasing to push the handles the tips of the wings, under the action of the wind, go back

to the rear, and the operator comes down gently, repeating this operation by next rising half a meter (20 in.), then a meter (40 in.), etc. When the experimenter has become accustomed to this slight fall he may go farther—that is to say, he may carry the tips of the wings farther forward and with more energy, and then attempt to rise 10 ft. and go and fall in the water.

The extent of ascent will thus be controlled perfectly except for the irregularities of the wind gusts. This irregularity will be partly regulated by the sensation which the current produces upon the aviator, as he knows that the wind gust raises him, and as he knows also that in order not to be carried too high, or even to hold himself at a level, it is only necessary to allow the tips of the wings to move to the rear so that they will no longer act, he will instinctively perform this manœuvre. It is probable that the experimenter will not become frightened, especially if he is only 1 or 2 meters (3 to 6 ft.) above the water, and that he will finally appreciate the safety of this manœuvre. Experiment has shown me that the wings are carried very readily and without any help to the rear under the action of the pressure of the wind when they are long as compared with their breadth, and also (and this should be remembered especially at the time of trial when the wing plane is nearly horizontal) that, whenever in rising the angle of the apparatus becomes 30°, the wings no longer respond to the action of the wind. It is then necessary to aid them, and that energetically, by forcing their tips to the rear.

I have always been disagreeably surprised at the instant of this rising. In the beginning I was ignorant of this effect, but I learned to my cost; and besides, I did not experiment with an aeroplane with long and narrow wings, a shape which it is absolutely necessary to use in the first trials in order to prevent overturning unawares.

But before even this much is attempted it will be best to test the effect of these various manœuvres by securing the apparatus and its rider against excessive motion in any direction.

For this purpose a location may be selected upon a pier, with water on both sides, and broadside to the prevailing wind, or upon a vessel with at least two masts, anchored at a suitable distance from the shore. Then a line or cord is to be fastened to the front of the machine and carried to windward as high a point as it is possible to reach. The line should be 300 ft. long if at all practicable.

By attaching a similar cord to the rear and two other lines of equal length to each wing tip, the aeroplane being carefully faced against the wind, there will only be possible a single slight motion, that of the rising allowed by the slack of the cords.

We have now our poor aeroplane sadly hampered; this apparatus, which is to confer liberty of transportation through space, which is to make man independent of the glebe, needs no less than four ropes to enable the aviator to become accustomed to its uses; but when thus tethered in every direction

it is to be hoped that there will be experimenters who will dare to try it.

The first act of flight which the aviator must then study—the only one, in fact, which the limiting cords will permit—is ascension directly against the wind; and this is simply the one act most difficult in flight, it is that of the bird of prey when under the most favorable conditions in the sky. This will be much facilitated by means of the four cords which control, in the four directions, all possible horizontal movements. It will then be only necessary for the aviator to possess sufficient adroitness to use the vertical component of the wind pressure, and this will be comparatively easy to learn. There will be found among the amateurs after awhile some skillful ones who will execute this manœuvre to perfection.

When assurance has succeeded fear, the experimenter will be able to release first the cord at the rear, and this will permit him to obtain a slight forward movement, produced by carrying the tips of the wings to the rear by an amount which is a little greater than that required for immobility.

In order next to supersede the effect of the two cords which are attached to the ends of the wings, it is necessary that the apparatus should have powerful means of steering it horizontally, so that the aeroplane may be quickly turned when it shows any tendency to leave its direct course against the wind.

There would then remain only the first attachment, that which prevents the backward motion, and the cutting of the first three cords will now allow of some very interesting evolutions toward the front.

When the experiments are transferred to the water—for next we must come to that—then the aeroplane may be detached from the front cord of 100 meters (328 ft.) long, for it will then be known by experience that it possesses the faculty of going forward. All the other manœuvres will then become easy, since the aviator will have become accustomed to the sight of empty space below him.

In order to resist a rapid current of air while upon the ground it is necessary to face the wind, and to have the points of the wings well to the rear; in any other position the aviator will be overturned. Look at the bird; his beak always shows the direction whence the wind comes. If, through youth or inexperience, the bird is caught by the wind from the rear he tumbles over. This awkwardness only occurs in domestic birds; the wild birds never make such mistakes.

The one great element of success is to take no chances of accident. I have always said that practical aviation will be solved only by a timid man—I might almost say a coward—but one who is reflective and ingenious as well as timorous; one who will accumulate in his favor all the elements of success and carefully eliminate every element of accident. How can a man, a poor grubbing creature, entirely ignorant of aerial manœuvres, launch himself at once into empty space hitched to an unnatural machine which he does not know how to handle, and hope to succeed at a first trial? It is absolutely necessary

to proceed slowly, and instead of trusting everything to chance, we must gather upon our side every element of success. We must exercise prudence, sagacity, investigation and good judgment. Without these we will only succeed in breaking our necks, and broken necks do not advance the cause of aviation.

When these preliminary experiments have resulted favorably and indicated that control over the apparatus can be obtained, it will still be far from being perfected, and now the experimenter can call other inventors to his aid who may have worked on the same lines, and solicit the assistance of capital.

Capital will certainly not be easily obtained for the experiments, but it will probably be easier to overcome that difficulty than it will be to overcome the secretiveness of inventors. In point of fact, the inventor is a very curious individual to study, and especially when he is but little informed of the question. But we will leave alone this class, to whom knowledge will come slowly, and address ourselves to those who for some time have perceived the coming light of day, who have already tried their powers, and from whom illusions vanished years ago. It is probable that among these we may be able to gather together a group of different aptitudes which, when well united, will aid each other with their special gifts and knowledge, and valuable results may thus be accomplished.

In practice some will be investigators and designers, others be the blacksmiths, mechanics, joiners and aviators.

Now, if this group work together harmoniously, concealing nothing, and grasping the whole problem, which is already ripe and well considered, they ought to arrive at a satisfactory result. Combination is essential, for one man cannot be everything at the same time.

The shop in which the machines are to be built should be thoroughly well equipped with special tools, and should employ one or more expert mechanics familiar with the subject—ingenious men, who would be able to extricate the aviators from the mechanical difficulties which they encounter at each step when seeking to produce particular movements in order to perfect their machines.

All financial arrangements should be perfected in advance, so that there would be no question as to the sufficiency of the funds provided to perfect the machines.

Not only should the group of aviators have their minds set at ease, but they should be provided with special conveniences and with the locations for constructing machines and for trying experiments. The workshop in which to build the machines is a matter of no marked peculiarity. It is simply a large shop situated near some great center, so as to obtain supplies readily and lose as little time as possible. But the spot in which to try the experiments imperatively demands special conditions, which nevertheless are easily obtained.

It would be best located on the borders of the Mediterranean or the Gulf of Mexico, which are warm seas. There a property should preferably be obtained upon a bluff headland, so as to be able to plunge down into space, and boats should be

provided. A small workship well equipped should be erected to repair mishaps and breakages which are sure to occur. It will be best that there should be a physician in the party, because this will create a certain amount of confidence, for it will not seem as if one were entirely abandoned.

Certainly, a company of such aviators, well selected, provided with all needed facilities for experiments and construction, would accomplish excellent work. They would build at the main shop various kinds of machines in the winter and try them in the warmer season, when involuntary baths would not be so disagreeable. These experiments could be carried on under all conditions until the machine was perfected ; and this ought to be done very much sooner and much cheaper than if a single man tried to work it out alone. The cost, therefore, would probably be much less than the French Government has already spent on its war balloons. If a quarter of the amount expended on military balloons had been devoted to the solution of the problem of aviation, I believe the object would have long since been achieved. It will be solved, there is no doubt, for, after all, it involves no greater undertaking than to invent and develop the bicycle. The only difficulty to be overcome is to become familiar with empty space, and it is in order to conquer the involuntary fear which it occasions that there is need of leisure, time, and money.

EXPERIMENTS WITH HEXAGON AND TAILLESS KITES.

By William A. Eddy.

In 1890 I began experiments to determine the relation between the width and length of the ordinary kite. My object was to evolve the best form of kite to be used in raising self-recording meteorological instruments to a great height, because many important problems in meteorology would be affected by investigations of the upper air currents.

I first found that a narrow kite would not remain well in the wind. It made a swift movement downward to the right or left, while a short, broad kite, if properly weighted with tail, would fly well. It was evident that greater lifting power and kite surface could be obtained by filling in the points of the well-known six-pointed star kite, thus making a hexagon or six-sided kite. The lifting power and steepness with which the string reached up to the kite were remarkable. In certain winds this kite might have approached the zenith had not the weight of the tail held it back. This lifting power suggested the possibility of attaining great altitudes with a large kite. But a very large kite must be flown with a heavy rope throughout, while, if other kites were attached to a main line, then heavy rope would be required near the earth's surface only.

In putting the lighter line aloft there is an obvious saving of weight, as well as of power to withstand the strain of additional kites. In this way I distributed the strain, caused by relatively small kites, at selected points making it adjustable and measurable. If one kite were used, then by the time it had reached an altitude of say 2,000 ft. with 3,000 ft. of string paid out, the weight of the string would become so great in proportion to the pulling power of the kite that the string would slacken, and thus cause the kite to recede, but rise no higher. Lightness and strength of string are required to attain great heights. I raised the sagging line by so flying nine kites from one string that the greatest pull was exerted upon the heavy cord near the ground, and lightest pull upon the lightest cord aloft. As the string below each kite was very steep, it was fastened to the more gradual ascent of line leading upward to distant and very high kites. The slacking line was thus lifted interminably by additional kites at intervals of 600 or 700 ft. The slacking line was also raised by putting on more kites at the ground as the main line was paid out. This principle is illustrated by the fact that if the point at which the kite is held is raised by other kites, then it is as if a man holding the kite increases his stature to 500 ft., causing the kite to rise 500 ft. higher.

On May 9, 1891, four hexagon tail-kites had been used to lift the slacking line of a fifth and highest kite. The last twine paid out was cable-laid, having a breaking strain of about 74 lbs. The steepness of the line at the ground was about 30° from the horizontal and half way up to the top kite, which was doubtless about 4,000 ft. above the earth; the angle of the main line was about 45°. There was no time for anything but approximate estimates and averages. The highest kite of the five, about 2 ft. in diameter, was so far away that the eye at times lost trace of it during several minutes—not because it was invisible, but because the eye did not look in the right direction to see it. The distant speck was not readily found when once lost sight of, even in a clear sky. The kite was covered with dark blue paper, otherwise it would have been still more difficult to discern it. As this highest kite was over the eastern part of the Kill von Kull, a branch of New York Bay, it was at the moment impossible to do more than relatively measure its height. The general conviction of the spectators was that it was about 4,000 ft. high.*

The limit of altitude attainable by means of tandem kites is yet to be determined. The 1891 experiment convinces me that if several miles of twine were extended upward into space the steep slant of the line could be maintained by increasing the size and number of the kites. With 10 miles of line and 50 kites from 2 ft. to 8 ft. in diameter, an altitude of 4 miles ought to be reached, unless the rarefied air should fail to exert sufficient pressure. The elaborate observations of Clay-

* Recent experiments and measurements in 1894 indicate that the kite of May 9, 1891, may have been 6,000 ft. high.

ton, the cloud expert of the Blue Hill Observatory, Mr. A. Lawrence Rotch, Director, have reaffirmed the fact that there is an increase of wind velocity at high altitudes as denoted by the measured motion of the *cirrus* clouds which float at a height of 8 or 9 miles, as shown by Clayton. It is therefore reasonable to assume that the kites would have ample wind to sustain them.

When the tandem strings of kites are flying, I have often noticed astonishing variations in the directions of air currents. Once when a string of kites was very high I observed that the upper kites described a sharp angle of direction as related to the lower, and when the kites were drawn in this sharp angle disappeared as the earth was approached. In case an altitude of 6 miles were reached, this variation in current might imperil the main line of kites by carrying all the upper ones away from the lower in an opposite direction. The variation in air currents may be a serious obstacle at times; but my experience during several hundred kite ascensions, both with and without a thermometer, is that a reversed current is exceptional if the kites are high in the air. I have observed only two reverse movements during two years at Bayonne, and these were at the surface. The change of current to a right angle from the original direction will promptly carry the kites with it, especially if the kites are tailless. In one instance the wind suddenly reversed and carried a Malay tailless kite in the opposite direction without causing a descent, although most of the string slacked and lay upon the ground while the kite was slowly changing its direction. Of course a gradual change of current during several hours will readily veer a line of kites along in the new direction.

In time I think the Malay kite will attain an altitude equaling that of the highest balloon ascensions at a relatively small cost, and that it will remain with its recording instruments several days in the *cirrus* clouds at a height of 9 miles. This may call for many decades of experiment. A balloon floats and drifts away and endangers life, but a kite, or line of them, will remain nearly stationary and enable self-recording instruments to give us invaluable data at a great height above a local point and during hours, and possibly days. The law of the upper-air movements can be mastered only by an incessant waste of kites, owing to breakage and loss. At present great heights can be reached only during daylight; and if time is lost in adjusting kite-tail to various wind velocities, then night may come on and the experiment be further delayed by the necessity of attaching lanterns to the strings below the kites. With a strong tandem line of large kites perpendicular cords can be run up to an upper kite, and the instruments raised and lowered, unless the altitude is too great.

The fatal delay due to attaching kite-tail I have obviated by reinventing the Malay tailless kite. Mathematical calculations were unsuccessful, and progress was made only when the kite was submitted to the wind. By trying curved sticks of many lengths, in proportion to the expanse of the kite, different de-

grees of tension that relaxed before the wind, different adjustments of rigid and pocket-like coverings, a form was finally found that would outfly the hexagon kite, equal it in lifting power, and fly with a steeper string.* These tailless kites remain to be further perfected in their adaptation to winds of more than 30 miles an hour. The absence of tail makes it possible to fly them in groups within 10 ft. of one another. If they collide they reverse with a graceful curve, and again at once take their regular flying position. As the cross-stick of this kite must be flexible, to enable it to adapt itself to changing wind velocity, it is consequently subject to fracture. Bamboo may be too flexible, and spruce and white pine too rigid. The best results are attained by combining spruce and bamboo. An adjustable steel cross-piece, the flexibility of which could be gauged in accordance with the violence of the wind, with lead at the center of gravity to make it plow its way through gusts, would probably enable this kite to successfully fly in a 60-mile wind. Light-wind kites, in groups of three, with light sticks and tissue paper, will fly at a height of about 1,800 ft. in a dead calm, if the person holding them walks at the rate of 2 or 3 miles an hour.

At twilight during July and August I have many times noticed that while no wind was moving near the ground, yet the lower clouds were moving rapidly. With a long string laid along the ground to the kite I have, by walking slowly backward, raised the Malay kite to an upper current during a calm at the surface. The vertical thickness of a mass of sluggish air may be often penetrated, yet when the calm air extends to a considerable height it may be difficult to get above it. A rapid walk of 2 miles would doubtless bring success, particularly at twilight in summer when the night winds set in. The vertical thickness of calm air may be sounded upward to a great distance by rapid motion at the surface. At night in summer the upper winds are steady, while a dead calm prevails at the surface of the earth ; and when the day breeze is inadequate, then these evenly flowing night winds are sufficient. During several years I have not observed a continuous twenty-four hours when an upper current could not be reached, as evinced by the motion of tree branches. The kites rarely fly in precisely the same direction, owing to differences in the angle of inclination. The upper currents are revealed by a difference of direction, as compared with the direction manifested by the same kite at the ground. In the rare case that any two kites happen to fly exactly alike, they can still be maintained aloft by placing one kite a few feet farther away than the other, upward along the main line.

On the nights of June 2 and 3, 1893, a kite penetrated a cloud coming in from the sea and remained invisible some time. In August, 1892, the kite penetrated a shower cloud for a moment—it faded away and then came forth—but as the kite was

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* For an account of this kite as an aeroplane, see AERONAUTICS, March, 1894.

of paper it was necessary to haul it down. The kite of June 2 and 3 was about 4 ft. across, tailless, of paper, and rose to a height of about 1,200 ft. The clouds had probably begun to descend, but the surface was not foggy until toward midnight, the kite having been sent up about sundown. Water-drops collected on the string near the kite while aloft, but at the ground the string was perfectly dry. This kite while in the air was soaked by mist, which caused it to slowly descend.

The success of this tailless kite depends upon its surface being adjusted with extreme exactness. In case too much surface appears on one side the kite will fly sideways and work its way out of the line of the wind. This can be partly remedied by attaching small weights at one side of the kite; but if the surfaces adjoining the central spine are too much out of balance, even the adjustment of weights will not make the kite fly.

A further series of experiments with perforated Malay kites will be carried out during 1894. So far I have experimented by cutting the outline of a small kite out of the center of a large one. This lessened the wind pressure during high winds and caused increased persistence of movement. Extreme accuracy is called for, however, if a large opening in the kite is made, because a mistake of an inch will cause a side movement that will carry the kite downward. The late Mr. C. W. Hastings made an important suggestion regarding the management of kites in high winds—that the kite be proportionately and slightly weighted with sheet lead at its center of gravity, thus giving it inertia in high winds and resulting in stability. As before mentioned in this paper, I tried this method in the summer of 1892, when Mr. Hastings visited me at Bayonne, and found it successful. It remains to be elaborated in 1894.

The Malay kites have an advantage in that when placed face downward on the ground they rise into the air if the string be pulled suddenly. They thus do not require to be held when making an ascent, except at times in strong winds. When hoisted from the ground by other kites, they take their flying position without further adjustment. In a calm this kite seems to hover with fixed wings like an albatross, and when it finally completes its gradual approach to the earth it is ever ready to rise with a quick pull of the line.

In 1892 I read that some Chinamen flew kites at the base of Washington Monument, Washington, D. C., with the object, I suppose, of using the shaft to measure the altitude of their kites. Since the monument is 555 ft. high, it would of course admirably serve the purpose of a long unit of measurement. But the part of their pastime that interested me most was that they cut holes in their kites to make the movement steady. It seemed to me that if perforation produces steadiness, then the same principle might apply to flying-machine planes. I experimented with paper planes and found that a narrow-pointed plane, if given a convex under surface, might fail in equilibrium if launched with an impetus. But if a long, narrow piece of paper were cut out of the center, as you might cut

the bottom out of a boat, and along nearly its entire length, then the same plane, if weighted, will glide easily and swiftly through the air in the direction in which the first impulse is given. As about one-fifth of the plane is cut out from the center, it follows that there is gain in lightness for flying purposes. A small model of this plane was mailed by me to Mr. Octave Chanute from New York City on June 5, 1893.

SOME EXPERIMENTS WITH KITES.

BY J. WOODBRIDGE DAVIS.

THE kites experimented with are foldable and steerable.

CONSTRUCTION.

Their construction is as follows:

The framework consists of three sticks of equal dimensions, pivoted at the center, free to turn on the pivot. The sticks taper from center to ends, according to formulæ involving the wind velocity, length and material of sticks, the safety factor, and position of fastening the bridle strings. Computations were made to ascertain and provide for the bending and shearing strains at every tenth part of a semi-stick. From these was derived the following table for determining the dimensions and weight of the kite stick. As the shape of the kite, the material and safety factor, and the position of fastening of bridle strings is constant for all the kites, the dimensions and weight vary only with the wind velocity and size of kite.

v = velocity of wind in miles per hour,
L = length " stick " feet,
b_1 = breadth " " " inches at center,
d_1 = depth " " " " " "
b_2 = breadth " " " " " ends,
d_2 = depth " " " " " "
W = weight " three sticks in pounds.

v =	10	15	20	25	30	35
$b_1 \div L$ =	.0482	.0661	.0826	.0983	.1133	.1277
$w \div L^3$ =	.0009	.0017	.0027	.0038	.0050	.0064
v =	40	50	60	70	80	
$b_1 \div L$ =	.1417	.1685	.1942	.2190	.2429	
$w \div L^3$ =	.0078	.0111	.0147	.0187	.0230	

$d_1 = \frac{3}{4} b_1 \qquad b_2 = \frac{1}{2} b_1 \qquad d_2 = \frac{1}{2} b_1.$

This table is based upon Smeaton's coefficient (.005) for wind pressure upon a plane surface at right angles to the wind. To connect the table so as to correspond with Professor Langley's coefficient (.00327), multiply each tabular value of b_1 by

$$\sqrt[3]{\frac{327}{500}} = 0.868,$$

and each tabular value of weight by

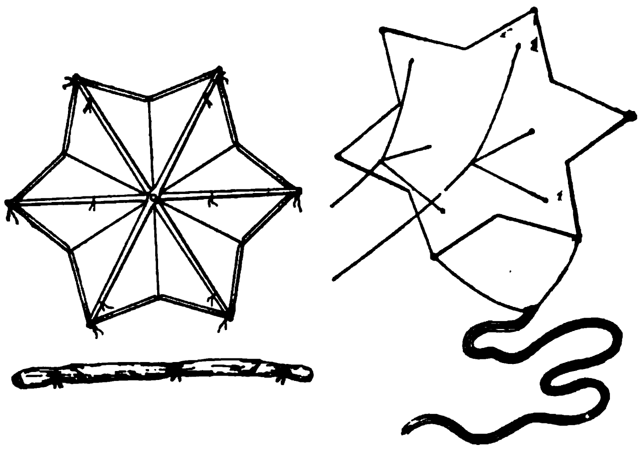
$$\left(\frac{327}{500}\right)^{\frac{2}{3}} = 0.753,$$

the velocity remaining the same. For the same dimensions the wind velocity may be multipled by

$$\sqrt{\frac{500}{327}} = 1.237.$$

The actual weight of a 7-ft. kite with cover and rigging, designed for a 40 mile wind, according to Smeaton's coefficient, or 50, according to Langley's, is $6\frac{1}{4}$ lbs. This kite will safely bear a wind pressure of 250 lbs.

The material used is American ash.



The safety factor is 2. on supposition that the kite is held perpendicular to the wind. This is about 3 when the kite is inclined in flight. When one bridle string is parted the safety factor is 1.2.

Shoulders are cut at the ends of the sticks to accommodate the tying strings of the cover. Shoulders are also cut at six places on the three sticks to accommodate the bridle strings.

The cover is made of oiled fabric similar to that worn by fishermen, being the lighter, and less heavily oiled. This material is very stout, flexible, wind and water-proof. For winds under 20 miles ordinary oiled muslin is used.

The shape of the cover is that of a six-pointed star. It is

planned as follows: A regular hexagon is inscribed in a circle, its vertices being the points of the star; then a regular dodecagon is inscribed, having alternate vertices coincident with those of hexagon; then the remaining vertices of dodecagon are drawn in toward the center as far as they will go, leaving the sides of dodecagon unchanged in length. Thus we have a reëntrant regular dodecagon, which is the shape of the kite. Each salient angle is exactly a right angle. The area of this figure is 0.55 of the square of one stick. The reasons for adopting the star shape instead of hexagonal are that with the former the kite seems to float more steadily and the cover to stretch better.

A stout binding cord extends around the perimeter. At the points strings fastened to the binding cord are ready to be tied to the ends of the sticks. Six openings are made in the cover to allow the bridle lines to pass through to the sticks.

The ordinary method of rigging a kite for flight is to bring the bridle lines attached to the sticks all to one point, and connect them with a single flying line. With such arrangement the kite cannot be steered. The plan here adopted is to bring the three bridle lines on the left side of the kite to one point, where they are tied to a ring in such manner as to be easily changed in length; and, in the same way, to bring the three bridle lines on the right side of the kite to another ring. To these two rings are separately fastened the two flying lines, when the kite is to be raised. The bridle strings are fastened to the sticks in two vertical lines. Investigation shows that the positions for fastening, corresponding to maximum strength and lightness of framework, are for the inclined sticks, as measured from the kite's center, at $\frac{73}{100}$ of the length of the semi-stick, and for the horizontal stick at $\frac{36}{100}$ of the length of the semi-stick.

A loose band connects the lower two points of the kite to receive the tail; a lighter band connects the top points to receive the top line.

To fold the kite, untie four strings at four of the star points, leaving those attached to one stick tied. Rotate the sticks until they coincide in direction, and roll the kite up. To spread the kite, reverse this operation.

The tail used is common clothes-line loosely looped at intervals, and attached in 100-ft. lengths as required.

The top line is light and strong, such as is used by masons in lining up walls.

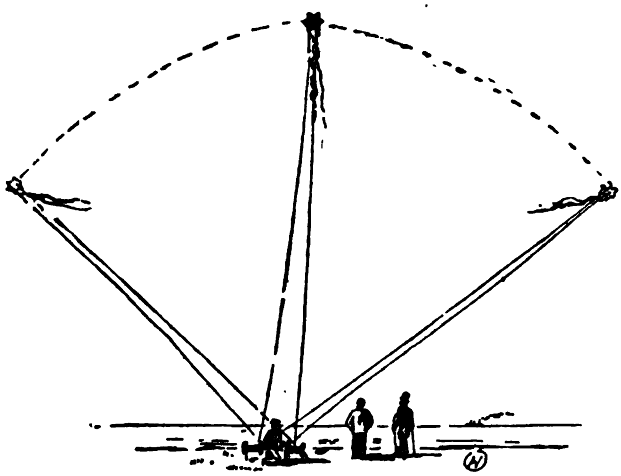
The flying lines are manilla, tested to bear 350 lbs. strain each, and are paid off from two light wooden reels, screwed to a platform pegged to the ground or lashed to a yard-arm. The reels have cranks, band brakes, ratchets and pawls.

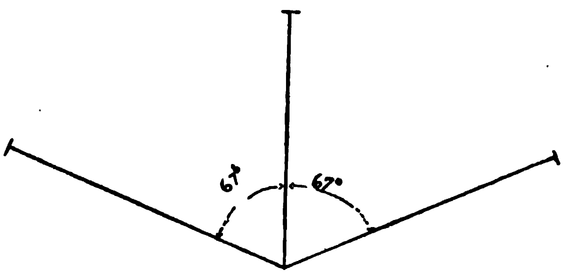
These lines are suitable for winds blowing from 20 to 100 miles per hour.

EXPERIMENTS.

First, the kite was flown without top line. By reeling in the right-hand flying line the kite was caused to veer to the

right of the leeward point. By paying out the right line, or reeling in the left, the kite was caused to veer to the left. The deflection on each side was 67½°, or 135° in all. As the





kite was brought off the wind its altitude diminished, so that it moved laterally in a curve like that of a rainbow.

The kite steadily rotated on an axis through its center perpendicular to its surface, as it was deflected, so that it always pointed head first to the wind, the tail streaming out to leeward. At its extreme deflection the axial line of kite and tail seemed to be almost horizontal.

Next, the bridle strings were altered in length, so that the kite had no slant—that is, its surface was perpendicular to the flying lines. The top line was attached. When the top line was slack, the kite would not rise. When the top line was drawn in, the kite was caused to mount to a point almost overhead. The kite could be lowered until its tail touched the ground, and then raised again. By using the top line and one flying line, the other being clamped, the kite was made to describe curves in the air, and was found capable of lining out any plane figure. A rowboat was sent out and stationed 40° or 50° away from the direction of the leeward point: the kite was then directed over the boat and lowered, a message was pinned to the tail, and the kite caused to rise and travel to another place. This performance was rapidly executed.

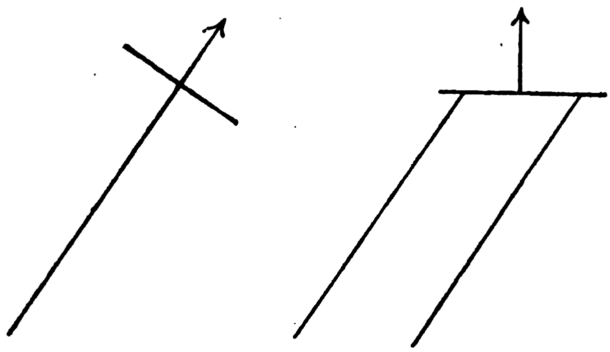
A wagon was specially constructed with wide, low platform. The passengers sat on the floor and hung their feet down in wells, in order to keep the center of gravity low. Two reels were fastened in the rear, and the lines ran thence through pulleys lashed to the right and left rails near the front. A kite was attached with top line and two flying lines. A man in the rear manipulated the reels; a man in the front steered the wagon by means of a pilot-wheel on a vertical shaft, and attended to the brakes. The top line was manipulated to increase or diminish the power as the kite was lowered or raised, and to keep it from falling when the wind slacked, in which service it proved efficient. The wagon containing two passengers was drawn up and down through the sand upon the sea beach at Arverne, L. I., last summer, with the wind abeam, at a rate of speed about equal to that of a horse-car, by means of a 7-ft. kite having 27 sq. ft. of surface. A 10-ft. kite, of 55 sq. ft. surface, has since been made for the wagon, in order to increase the speed; but it has not yet been used.

A mile of light rope was wound on a reel and carried to South Brother Island. A loop was made in the forward end, and 20 ft. back of this was slung on a 32-lb. wooden buoy. A 7-ft. kite was raised. When about 400 ft. had run out the two flying lines were cut. One was immediately fastened to the loop at the end of the mile rope; the other was also fastened there, after being drawn in so as to direct the kite to a pier on Riker's Island, five-eighths of a mile distant. Then the kite was let go, and it dragged the buoy and line to the pier in 12 minutes through a strong cross tide. Although the tide bore the middle of the line down stream, it seemed to have no effect upon the course of the buoy.

The next experiment was tried on Brenton Reef Lightship, which is moored 1¼ mile from the extremity of Brenton's Point, a tongue of land at the mouth of Narragansett Bay. The problem was to send a line from the ship to this point through any tide. The outfit was the same as that used on the island, except that the life-line was 2 miles long, made of ½-in. manilla, quoted to bear a strain of 780 lbs.

The first trial, however, was made to seaward in a northerly breeze, the principal object being to determine with what

facility the kite could be raised from shipboard. It was found that this could easily be done. The top line was rove through a small pulley before being fastened to the top band of the kite. The pulley was hauled up on the flag halyards



and the halyards wrapped around the mast cleat. A man held the kite at the rail, the pressure being taken up by the taut flying lines.

At the word the top line was hauled in a little, hoisting the kite, and the flying lines were paid out. Instantly the kite was sailing above the level of the masthead. The top line was slacked and the kite started to fall, its tail being too heavy. The kite was drawn in, supported by the top line, and a portion of the tail was cut off. It was launched again as before and found to be self-supporting when the top line was slackened.

Repeatedly the experiment of taking in and letting out the kite was tried. It was surprising how easily this could be done. The top line steadied the kite on the same principle as the tail, besides supporting it until it could be made self-supporting. The flying lines steadied the kite laterally like guy lines, besides steering it, so that it could be accurately and steadily drawn in or let out through a space in the rigging scarcely wider than itself.

This experiment indicated that it is easier to raise a kite on shore by means of a flagstaff than in the ordinary way, and that kites can be readily flown from cupolas and towers surmounted by flagstaffs.

It was also learned that even when a kite is flown straight to leeward it is better to use two flying lines than one. With one line the kite was unsteady in its movements; with two there was not the slightest tendency to lateral oscillation. This is explained theoretically as follows: When a kite flown with one line is a little off the lee point, its surface maintains the same angle with the flying line, and the wind pressure,

being normal to the surface, does not tend to drive it back to leeward. It is driven to leeward merely by the action of the wind upon the edge of the kite, and by the friction of the wind blowing along its surface, both of which forces are feeble when the deflection is small. But a kite flown with double lines remains parallel with its original plane when deflected, and the full force of the wind pushes it back to the leeward point.

The life-line was sent out in various directions, and finally the kite was directed straight to leeward, and the line sent out 1 mile to sea in 18 minutes, the line being marked with a leather tag at each quarter mile. The first quarter ran out in two minutes, although two men were braking the reel with staves. The second quarter was not timed, as we stopped the line to try its pull, which was more than one man could hold. The third quarter ran out in four and one-half minutes, and the fourth in eight minutes. It was noticed that whenever the line was checked the kite immediately lifted the buoy 50 or 60 ft. out of water, and dropped it again as soon as it resumed its headway. It is believed that the same effect would be produced by obstructions, such as floating spars, reefs, and bars. The line was hauled in by five men working at two cranks of the life-line reel. The kite remained flying until brought to the deck. Next the kite was dropped over the ship's side into the water, and by hauling on the lines was raised and put in flight. It was found that this could be done with the flying lines alone without the top line, by hauling on one only, which raised the kite partly and allowed the wind to sweep under it and lift it out of the water and put it in flight.

Last April in a southwest wind the kite was carefully aimed for Brenton's Point through the strong cross-current of an ebbing tide. When half a mile of line had run out a second kite was attached to the line as an auxiliary. The forward kite succeeded in landing the buoy and life-line about 20 yds. east of the Point.

Last June the keeper of the lightship attached one 7-ft. kite to the end of a 200-fathom 5-in. hawser, without a buoy, in a 25-mile northerly breeze, and sent the entire hawser to sea against a strong flood tide in 12½ minutes. Whenever the paying out was suddenly stopped the cable immediately leaped clear of the water.

It was the intention to measure the amounts and directions of the various forces acting upon the kite in the above experiments; on several occasions measuring instruments were carried along. But the interest on each occasion centered in some practical performance of the kite, so that no measurements were made.

AEROPLANES.

From the behavior of large kites a few suggestions may be drawn, which are perhaps applicable in the study of free aeroplanes.

The equilibrium of a body may be considered with respect to three axes at right angles to each other. In case of a kite or aeroplane, one may be taken normal to its surface, another coincident with the longitudinal axis or axis of symmetry, and the third at right angles to this in the plane. On account of the support given by the bridle lines a kite possesses in a marked degree the power of maintaining its equilibrium with respect to any axis in its plane; but about the normal axis. the ordinary kite, unprovided with a tail, is utterly helpless to prevent rotation. This defect is completely remedied by the attachment of a long light tail. It is found that the top line also steadies the kite, even when the tail is insufficient, and that the kite is never so steady as when the top line, which corresponds to the motive power in the aeroplane, is tugging at one end, and the tail at the other.

The natural inference to be drawn is that the motive power of a flying machine should be applied at the forward end, and that a drag should be trailed from the rear.

Both parts of this arrangement are inconvenient, it is true, and detrimental to the speed; but the advantage is obvious. We remark how steady and straight is the course of a rocket; and we can imagine how erratic and short-lived it would be if the power were applied at the rear end of the stick. The accuracy of the flight of an arrow is due to the fact that immediately after it is sprung from the bow the power accumulates in the head, and the feathered shaft acts as a drag. It is theoretically possible that when a plane is driven ahead of its motor, a good rudder can overcome the quick and powerful tendencies to deviation by continuous and instantaneous responses; but this is beyond the voluntary powers. With the power in front, and feather edges or a drag in the rear, the erratic motions become so tempered that they can easily be controlled by the voluntary powers.

The first animals to fly were the ancient reptiles; they retained their long tails for ages after they had acquired wings. The motive power was near the front; a drag was in the rear. The descendants of these monsters have spent a million years, more or less, in perfecting their organs of flight; it is probable that their ancestors could not have maintained an equilibrium with such delicate apparatus as is possessed and automatically managed by the modern bird. It is fair to judge from this that the initial successes of flying machines will not be made with those constructed upon the most refined theoretical basis.

This head and tail arrangement would seem to provide equilibrium about every axis except the longitudinal, or axis of symmetry. To attain equilibrium about this axis the following well-known principles may be applied. The symmetry should, of course, be made as perfect as possible; the longitudinal elements should be made as straight as possible, and parallel to the main longitudinal axis of the machine, and to the direction of the action of the power; the center of gravity should be sufficiently far below the centre of buoyancy, and

the line of application of the power should be as nearly as possible coincident with the resultant of all resistances ; the two lateral symmetrical halves of the aeroplane should make an obtuse angle with each other, the concavity above, the convexity below.

An equilibrating power might be arranged to act automatically as follows : Insert a fan-wheel in each lateral half of the aeroplane, like a ventilator wheel in a plate of glass, as far as possible from the axis of symmetry, or mount a vane on a vertical shaft extending upward from the car. A pendulum in the car can now be made to act like a governor to turn on and off and reverse, a supply of power to the fan-wheels, or to deflect the vane, which then acts like the dorsal fin of a fish, and by either or both of these means resist rotation of the aeroplane about its longitudinal axis.

MANUFACTURING HYDROGEN GAS BALLOONS.

BY CARL E. MYERS, BALLOON FARM, FRANKFORT, N. Y., U. S. A.

I BUILT my first balloon in June, 1878. It was 27 ft. diameter, contained 10,000 cub. ft. of gas, and the envelope and valve weighed 90 lbs. The material was fine unbleached cotton cloth, varnished with pure linseed oil gum thinned with turpentine spirits ; seven coats being applied by machinery invented by me for my first balloon and afterward patented. It held gas perfectly, and was in frequent use during seven years. It occupied 14 days in its complete construction. I have pursued the same system ever since. During 1891 I made 60 hydrogen balloons of various sizes in 60 days. In 1892 I built one lot of 10 hydrogen gas balloons and nets in five days, an average of two per day, but this does not represent the possible rate, as with my facilities a very large number of moderate sized balloons can be built together in the same time.

I propose here to describe the methods by which these results are attained.

Five seasons ago, finding my space too limited, I removed from Mohawk to my present location, the " balloon farm" at Frankfort, Herkimer County, N. Y., where I found ample room indoors and out.

Our varnish is prepared in a low pit out-doors, sheltered from wind, and provided with water from a hose handy. Fire is applied underneath a large iron kettle, covered over, containing pure, raw linseed oil, which in four to eight hours, according to heat, is cooked to a gum, at a temperature at first sufficient to scorch a feather and finally to char a pine stick. At this point, requiring some experience to determine, all fire is removed from the vicinity, and upon flame bursting out afterward from the kettle's contents through spontaneous

combustion, water is applied to the bottom of the kettle till it is sufficiently cooled to extinguish the fire within, care being taken to admit no water to the interior in order to avoid explosion. The whole process is dangerous, even with experience. The oil, which previously was quite fluid, is by this means coagulated somewhat like the cooking of an egg, and should be jelly-like on cooling and have the elastic properties of rubber, with a loss of much of its disposition to "heat" like oil varnishes. This gum, thinned to the consistency of syrup, is placed in a sloping vat, within and above which are various rollers, scrapers, squeezers, and roller scrapers, acting by pressure of springs and weights. The fabric, silk or cotton, unwinding from a roller, descends into this vat, and reappears soaked with varnish rubbed and pressed into all its pores. All the other appliances are for removing this varnish as completely as possible by mechanical means, till finally only the slightest smear or film remains, and the fabric winds upon another roller pressed as hard as if of solid wood.

It is then hung on lines to dry as soon as it can be unwound, all the work being done in the open air, and only when bright, clear, warm weather prevails. This open-air manufactory is in a deep, sheltered glen at the rear of the balloon farm. At the bottom of this glen is erected a tall wire fence, with 20-ft. poles, around which a canvas is stretched like the sides of a square circus tent, to further protect from wind.

Inside this wires 100 ft. long stretch from side to side, the ends of each sliding by hooks over iron tubes supported head high from another series of posts. To these wires the varnished fabric is attached by clothes-pins. In case of rain all the parallel wires can be moved close together, sliding along on the iron tubes at each end, so that the whole may be easily covered in a few minutes.

This precaution is necessary to prevent spontaneous combustion, which would occur if the partly dried oil cloths were pressed close together or laid in heaps. No dryer is used, and 6 to 12 hours' exposure to bright, hot sunshine are needed to complete the drying. The cloth is then rewound and revarnished, again and again, until four or five films have been successively applied to each side and fully dried, the entire application making so little added thickness that a micrometer caliper hardly detects it, and the weight is not much increased, though the varnish is too thick to apply with a brush, as no more turpentine spirits is used than needed, its resin being a disadvantage. The theory of operation is that each successive coating of varnish plugs up and overlays the microscopic pores in the underlying coats, through which the molecules of hydrogen might pass; and *any film thicker than hydrogen* is effective if applied a sufficient number of times, just as numerous plates of glass or wire gratings may obscure light—my theory of construction being that only "the varnish holds the gas, the cloth holds the varnish, and the netting holds the cloth." The features of advantage in my system are that any desired degree of impregnability to hydrogen can

be obtained with a single thickness of thin light fabric, thus ensuring a tighter and lighter balloon of any size, less likely to break in its folds than a heavily or thickly varnished balloon, irregularly coated by hand. The facility with which 1 or 100 gas balloons may be constructed simultaneously and speedily is another great advantage.

In cutting out a balloon two rolls of varnished fabric are unrolled together, so that the pattern half-segment may be applied on top, and the two half segments shaped simultaneously. These are afterward united by overlapping and sewing together the straight edges with a seam on each edge. These seams are then varnished on each side successively, and sometimes an additional thin surface coat is applied by hand brushes. As many of these completed segments are then united as may be necessary to complete the balloon required. The balloons are reinforced around the valve and top by several thicknesses of varnished and unvarnished cloth. When the "block system" of construction is used the segments are each cut several inches longer, to allow for the segment length being folded in ½-in. plaits crosswise at yard intervals and sewed on each edge or surface—the segments or half segments being so arranged that the bands may each go entirely around the balloon forming network, or to otherwise "break joints," like brick-laying. All seams are coated, either with thicker adhesive "seam varnish" or with thinner "balloon varnish," to ensure closing needle holes.

The netting I always build on a regular system of increasing size of meshes and weight of cord, and somewhat smaller than the actual size of the balloon, so that the net hugs the balloon envelope firmly, and practically meets all the strain like the framework of an edifice, the union being such that each portion reinforces another. The meshes reach nearly to the car, diminishing in number and ending in heavier cords and ropes, which pass through the loops of the basket ropes around the concentrating ring, which is thus not necessary to the integrity of the apparatus, but is simply a concentrator of lines. When broken this ring may be slid out of its fastenings or even used for ballast. My car is a platform of thin layers of cross-laid veneering, forming the bottom of a hammock netting bag, supported at its upper rim by a hollow metal ring, with cords connecting to the concentrating ring above. This platform serves as an inclined plane or rudder to partly guide the balloon's rise or fall toward any given point, as the aeronaut readily depresses any edge of it by placing his weight upon it. This feature, patented by my wife Carlotta, has been made of great use by her in ascensions outnumbering those of all other aeronauts combined in this country during the past dozen years with hydrogen gas.

The valve I use is of wood, of French pattern, and is the only feature in my system not original with me, so far as I can learn.

NATURAL GAS BALLOON ASCENSIONS AND CHANGE OF TEMPERATURE IN GAS BALLOONS.

BY CARL E. MYERS.

THE subject of temperature in gas balloons is an unknown feature in our text-books. A slight rise or fall in temperature is the speediest means of applying heat as a means of propulsion in connection with balloons. My first experiment in heating the gaseous contents of a balloon occurred at Franklin, Venango County, Pa., in 1886. It was also the first occasion, so far as I can learn, when natural gas from the earth was practically used for ballooning. Natural gas is a substance of uncertain quality, especially as to specific gravity, and I could obtain no data whatever as to its actual lifting qualities. To increase its levity, I decided to heat the gas. The experiment was, I believe, in every respect a novel one. The gas arose from the wells at a pressure of 700 lbs. per square inch. Traveling through 80 miles of pipe it reached the vicinity of the balloon very cold, and was checked by valves to 60 lbs. pressure per square inch. Here I passed it through 1,000 ft. of 1-in. pipe to a valve fitted on to a 6-in. balloon hose, which it entered with a roar like a steam whistle, and immediately distended to its full diameter, with a pressure feeling like that of a hose-pipe from a hydrant playing on a fire. To avoid the considerable fall of temperature which would result from expanding the gas into the balloon, I built a line of open fires at intervals along the 1,000 ft. length of 1-in. iron pipe, and so raised its temperature to about 150° F. at the entrance to the balloon, which held 10,000 cub. ft. of gas, and weighed with net and car 100 lbs.

I ascended slowly, carrying 20 lbs. of ballast. The balloon poised at 5,000 ft. elevation, expelling gas visibly heated and warm to the touch, flowing upward in tremulous waves from the wide-open neck. The balloon then descended 300 ft., and I closed the neck loosely with a loop of the valve cord about it. It then arose again to 5,000 ft., and again very slowly descended, landing gently 1 mile distant from the place of ascent and one hour after rising.

This balloon, filled with ordinary coal gas, had the week before carried me with 200 lbs. of ballast violently upward over 2 miles before being controlled by the valve.

My next experiment with natural gas occurred at the same place two days later. A balloon of less than twice the capacity of the former one was used, weighing, with the aeronaut, car, netting, and anchor, 300 lbs., besides 60 lbs. of ballast. The barometer was an aneroid, "compensated" for temperature, the same used by me just previously and found reliable on all occasions. Recessed within it was a delicate thermometer. The barometer had a vernier scale, with independent scale for feet and inches of atmospheric pressure and for altitude up to 20,000 ft., with a range of index to 1,000 ft. further.

The balloon, arriving late by express, was hurriedly filled

in one hour, and partly before the netting was properly adjusted about the valve, so that, as afterward found, a fold of the envelope interfered with opening the valve.

A brisk wind blew and the balloon was sent up violently to escape collision with a tall oil derrick near. The aeronaut, in this case my wife Carlotta, broke the valve cord in trying to open the clogged valve, and before the balloon could be checked by other means the index of her barometer had passed to its limit on the scale, so that the elevation may be safely stated as 21,000 ft., the highest made in America, or about 5 miles with natural gas. The balloon landed safely 90 minutes after rising, and 90 miles from its starting-place.

The point to which I wish to attract attention here is that theoretically so small a balloon with so heavy a gas ought not to have reached so high an elevation. It is a common complaint that balloons make higher ascents than are theoretically thought possible, assuming that the balloon contents are at the same temperature as the surrounding air at high elevations. The ready explanation of this is that the temperature within the balloon *is not* reduced to the outside temperature by expansion of its contents or by doing work, but that it is chiefly affected by conductivity through its envelope alone, and changes slowly, always lagging behind the outside temperature, when changing Thus the gas is warmer than surrounding air when it reaches its highest elevation. If it remains long surrounded by this colder temperature it cools off, shrinks in volume, and the balloon falls more or less rapidly, and may reach the ground with its gaseous contents considerably cooler than surrounding air, if it has at any time attained a cooler point; whereas, if it "*heats*" by compression or reduction in volume as it falls, it should be as warm as the surrounding air on reaching earth. It is a common experience with aeronauts that when once a balloon starts to come down from a high elevation, a considerable quantity of ballast has to be thrown out to check it, and this may be accounted for simply by the fact that the balloon has "cooled off" because of cold surroundings on high, and does not regain heat rapidly while falling. The query is, Is there a hole in the balloon, or is there a hole in our theories about the gas inside of a balloon doing any "work"?

As another illustration I draw upon my experience covering the third ascension made with natural gas, at Erie, Pa., the following year.

The balloon was that first used by me at Akron, O., for my swift fall, which was afterward enlarged to 18,000 ft. capacity. It filled in 40 minutes from a maximum pressure of 110 lbs., controlled by a valve at the balloon hose. The gas was lighter than that at Franklin and I did not heat it, and the contents of the balloon were consequently quite cold. I went up with 60 lbs. of ballast, and quite rapidly to escape a tall steeple, and arose steadily to 13,000 ft., when the balloon poised about 20 minutes and began its descent without loss of gas, except from its open neck. The temperature of air here

was 28° F. With the balloon neck closed I fell with gradually increasing velocity, and when within 1,000 ft. of earth I threw out all my ballast, 60 lbs., and nearly checked the balloon. I carried a photographic camera, and this I had wrapped in an inflated rubber life-preserver, covered with my overcoat and tied in a sheet. At 800 ft. elevation I let this drop in order to escape precipitation into a forest. When recovered it had its snap shot mechanism deranged, and two unexposed and one exposed plate broken and five exposed plates saved. Pretty good luck!

A fourth ascent with natural gas was made by me at Sandy Creek, N. Y., to test the capacity of a new well, and I think this included all ever made with this medium in this country. It is only serviceable with large or very light balloons.

FLOTATION VS. AVIATION.

By De Volson Wood, Member Am. Soc. Mech. Eng.

It is frequently the case that one is led to the special study of a subject by being requested to say or do something in its behalf. Such is the condition of the writer. A request to write upon it has led to the production of the following paper. Not being an experimenter in this line, he can only make use of the labors of others. Past experiments have been numerous and the literature considerable.

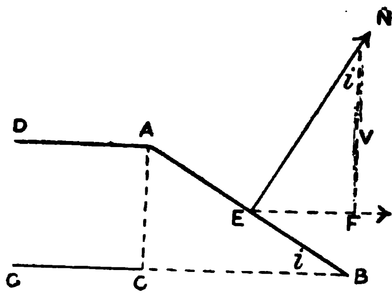
The question at this time is one of possibilities rather than of commercial success, and in this light many questions present themselves to the engineer for solution. Numerous inventions and discoveries have become practical by removing obstacles and improving upon first productions. The most important thing in an invention is to solve the problem in some way, however crudely, after which it may be made practical by improvements. Our problem requires that by suitable mechanism an air ship may be made to rise at any place and travel in the atmosphere in the wind against a moderate breeze. When this is once successfully done, the problem of possible navigation is solved. Drive a vessel across the Atlantic by steam power, even if it takes a longer time than by sail, and the possibility of steam navigation is assured. Even if it took forty days to cross the first time, improvements may reduce it to five in less than a century.

Similarly in regard to the locomotive, electric motor, dynamos, the printing-press, agricultural implements, rock-boring and other inventions too numerous to mention.

When an inventor undertakes the solution of a mechanical problem he should seek to adapt the means to the end. In doing this it may be necessary to break away from models furnished by nature and invent something peculiar for the case in hand. Take, for illustration, the history of marine navigation. When power was first applied to drive a boat, the web-footed and jointed leg of the goose were not imitated, but a

rigid oar was used; and when steam power was introduced the paddle-wheel, having a rotary motion, was invented to take the place of the reciprocating oar, and still further improvements made by introducing the screw or helicoidal wheel. Even if nature suggested a model for the last, it is so remote and far fetched that it may be claimed as a genuine invention.

When greater speed was sought on solid ground it was not obtained by a machine that would step, like an animal, although such devices were tried and abandoned. An invention was necessary; the rotary driver and steam blast made the locomotive a possibility; they were *genuine inventions*. In all these a continuous rotary motion has been one of the important elements insuring success. He who seeks to fly like a bird may find the "flying good enough, but the alighting very hard," as the man said who tried it. For these reasons I



would not consider with favor the idea of a practical, bird-like, flapping, mechanical flying machine. Such machines may serve as toys and objects for study, but not, we think, for transporting weights. But there are other reasons. As no fish furnishes a true type for a marine ship, so we might expect that no bird will furnish a true type for an aerial ship. They are at best only suggestive. The forms of the bodies of the fastest moving birds and fishes have been studied to see what lessons can be drawn from them. It is observed that both terminate at the rear in a rudder-like form, that of the fish being vertical and of the bird horizontal. The largest cross-section is somewhat in advance of the half length. Of the two, the bird form should be the more instructive, since it moves in the same medium as that of the air ship, and its appendages for producing flight are outside the body.

Discarding the flapping wings as a motor, the continuously rotating propeller is the only form of driver that commends itself. This may be used either to drive an aeroplane or a floating vessel. We first consider

AEROPLANES.

This mode of navigation is considered as an aviation or support by mechanical means only. If a plane inclined to the

direction of motion be pushed in air, there will result a normal pressure, and if the plane be inclined upward there will be a component of pressure supporting or tending to support the plane against falling. Such a plane is called an "aeroplane."

The following analysis gives the components of these pressures:

Let $D\ A$ be the direction of wind in the plane of the paper.

$A\ B$ the projection of the aeroplane on the plane of the paper.

S = the area of the plane $A\ B$.

P = the pressure on a square foot of area normal to the direction of the wind.

w = the weight of a cubic foot of air (which at the surface of the earth and at a temperature of 32° F. is about 0.08 lb.).

$i = A\ B\ C$, the slope of the plane.

$N = E\ N$, the normal pressure, $R = E\ F$, the horizontal pressure, and $V = N\ F$, the vertical pressure.

v = the velocity of the wind in feet per second.

Then:

$A\ C = S\ sin.\ i$ = the section of the stream of wind impinging against $A\ B$. The mass of air impinging, or impinged against, per second will be:

$$M = \frac{w}{g} S\ v\ sin.\ i.$$

Assuming that the resulting pressures follow the Newtonian law of a liquid jet impinging on a plane, we have:

$$N = M\ v\ sin.\ i = \frac{w}{g} S\ v^2\ sin.^2\ i = P\ S\ sin.^2\ i. \qquad (1)$$

$$V = N\ cos.\ i. \qquad (2)$$

$$R = N\ sin.\ i. \qquad (3)$$

As will be seen later, experiment does not establish equation (1) for a plane moving in boundless air; but whatever be the true value of the normal pressure, the vertical and horizontal pressures are correctly given by equations (2) and (3). At the surface of the earth, at the temperature of 60° F. and barometric pressure of 14.7 lbs. per square inch, $w \div g$ = 0.00237 nearly, so that

$$P = 0.00237\ v^2, \qquad (4)$$

$$R = 0.00237\ S\ v^2\ sin.^3\ i. \qquad (5)$$

Or, if c be miles per hour, then

$$P = 0.005\ c^2. \qquad (6)$$

For small slopes, $sin.\ i = i$ nearly, and $cos.\ i = 1$ nearly, giving

$$\left.\begin{aligned} N &= 0.005\ c^2\ S\ i^2, \\ V &= 0.005\ c^2\ S\ i^2, \\ R &= 0.005\ c^2\ S\ i^3, \\ U &= 0.005\ c^3\ S\ i^3, \end{aligned}\right\} \qquad (A)$$

where U is the work in pound-miles of a plane of small in-

clination to the direction of motion, moving with a velocity of c miles per hour in still air. All these values decrease as i the scope decreases, some as the square of i, and others as the cube. It also appears that any desired amount of supporting power V may be obtained by increasing the speed c and size of the plane S. If U be the work in foot-pounds per minute, c being miles per hour, then

$$U = 0.005\ c^3\ S\ sin.^3\ i \times \frac{5280}{3600} = 0.008\ c^3\ S\ sin.^3\ i. \quad (7)$$

But according to the experiments of Hagen, the resistance depends upon the form and size of the plane. His experiments were upon plane disks whose perimeters ranged from 7 to 25 in., moved with velocities from 1 to 4 miles per hour normal to the direction of motion. He gave the following formula:

$$R = (0.0028934 + 0.0001403\ p)\ S\ c^2, \quad (8)$$

in which

p = the perimeter of the plate in feet,
R = the resistance in pounds,
S = area in feet,
c = velocity in miles per hour.

The form of this equation was established by Borda, 1763.

This formula shows that the perimeter of large plates exerts a large influence upon the resistance. But it is not safe to apply an empirical formula to cases in which the data are largely in excess of those used in the experiments, and the surfaces used by Hagen are small compared with those necessary for a practical aeroplane. Indeed, this is a difficulty with nearly all experiments when applied to an air ship. The dimensions of the ship are so large that it cannot by asserted that the laws deduced by experiment are applicable to the case, and hence they can be used only as general guides. We will not, therefore, consider Hagen's formula, however valuable it may be for resistances for small planes moving with moderate velocities. It will be seen, according to equation (5), that the resistance to the forward movement of a plane in air varies as the cube of the sine of the inclination to the horizontal, so that the more nearly horizontal it is the less will be the resistance, and if appendages and thickness of the plane be neglected it approaches zero as a limit.

When the inclination is small:

$$\begin{aligned} N &= V = 0.005\ S\ c^2\ i^2 \\ R &= N\ i \text{ nearly.} \end{aligned} \quad (9)$$

To illustrate, take the design of a Mr. Henson in 1842, in which the area of the plane was to be 4,500 ft. and a tail of 1,500 ft., making an area of 6,000 ft., to carry 3,000 lbs. If the inclination of the plane be 10°, we have for the velocity necessary for supporting this weight:

$$c = \frac{1}{0.18} \sqrt{\frac{3000}{0.005 \times 6000}} = 55 \text{ miles per hour.}$$

This develops a horizontal force of $R = 540$ lbs. approximately, requiring a H.P. of

$$540 \times \frac{55 \times 5280}{33000 \times 60} = 80 \text{ nearly.}$$

If the slope were 5° it would require a velocity of about 110 miles to support this weight, developing a horizontal resistance of 270 lbs. approximately, but requiring the same H.P. as before, since half the resistance works over double the space in the same time. The resistance offered by the car and netting would add greatly to this resistance. Similarly if half the weight be carried with half the size of plane, it would require about 40 H.P. This gives some idea of the power necessary for securing high velocities with large planes.

The normal pressure, according to Hutton's experiments, may be represented by the empirical formula :

$$N = P\,S\,(sin.\ i)^{1.84\ cos.\ i\ -\ 1} \tag{10}$$

hence,
$$R = N\,sin.\ i = P\,S\,(sin.\ i)^{1.84\ cos.\ i} \tag{11}$$

and
$$V = N\,cos.\ i = P\,S\,cos.\ i\,(sin.\ i)^{1.84\ cos.\ i\ -\ 1} \tag{12}$$

These give :

TABLE I.

Angle of Slope, i.	Normal Pressure, N.	Horizontal Pressure, R.
5°	0.1303 $P S$	0.0113 $P S$
10°	0.2405 "	0.0417 "
15°	0.3489 "	0.1129 "
20°	0.5650 "	0.1561 "

A wind of 25 miles per hour will give, according to equation (6), a normal pressure of $P = 0.005 \times 25^2 = 3.125$ lbs. per square foot at ordinary temperatures and barometric pressures—call it 3 lbs. (Wolff on Windmills, p. 13). Then we find :

TABLE II.

For $i =$	At 25 Miles per Hour.		At 50 Miles per Hour.	
	$N =$	$R =$	$N =$	$R =$
5°	0.39 S	0.034 S	1.56 S	0.136 S
10°	0.72 S	0.135 S	2.88 S	0.540 S
15°	1.04 S	0.339 S	4.20 S	1.336 S
20°	1.37 S	0.468 S	5.52 S	1.872 S

If Hutton's law be true, it would require a plane of about 1,040 sq. ft. to support 3,000 lbs. with a velocity of 50 miles per hour at a slope of 10°, or about ⅙ the size of the former one. This would be a great gain in regard to size; but the *horizontal* resistance would be the same as before, being $N \sin. i$, or $N i$ nearly, so that it would require the same power as before to drive it at the same speed.

At 5° slope and 25 miles per hour, according to the former law, the supporting power would be about 145 lbs. for 6,000 ft. of surface, and acceding to Hutton's law $0.39 \times 6000 = 2340$, or more than 16 times that of the former. The former would require about 0.83 H.P., and the latter about 13¼ H.P. This does not include the resistance of the car and accessories, which must be considerable, but it impresses one with the importance of a knowledge of the correct law for normal pressures. The following empirical law of Duchemin is found to be practically exact for angles from 0° to 20°, and nearly so to 45°:

$$N = \frac{2 P S \sin. i}{1 + \sin.^2 i}, \qquad R = \frac{2 P S \sin.^2 i}{1 + \sin.^2 i}, \qquad V = \frac{2 P S \sin. i \cos. i}{1 + \sin.^2 i}. \tag{13}$$

These give:

TABLE III.

ANGLE OF SLOPE, i.	Normal Pressure, N.	Horizontal Component, R.	Vertical Component, V.
5°	0.173 $P S$	0.015 $P S$	0.172 $P S$
10°	0.337 "	0.058 "	0.332 "
15°	0.486 "	0.125 "	0.469 "
20°	0.612 "	0.209 "	0.575 "

According to Duchemin's formula, the normal pressures are larger than those given by Hutton, and hence the area of the aeroplane may be correspondingly less; but to sustain the same weight the power to drive it horizontally will be the same as before computed.

Thus, to sustain 3,000 lbs. according to this law at a velocity of 50 miles per hour at an inclination of 10°, will require:

$$S = \frac{3{,}000}{0.332 \times 0.005 \times 50^2} = 723 \text{ sq. ft.},$$

or less than ⅛ the size as given by the Newtonian law, and about $\frac{7}{10}$ the size given by Hutton's formula.

The horizontal resistance will be, at 50 miles per hour:

$$723 \times 0.058 \times 0.005 \times 50^2 = 524 \text{ lbs.}$$

The H.P. required to drive the plane with this velocity will be:

$$\frac{524 \times 50 \times 5280}{33000 \times 60} = 70 \text{ nearly.}$$

At the same slope, at a velocity of 100 miles per hour, supporting the same weight, it would require only ¼ the surface, or about 181 sq. ft., and the horizontal pressure would be :

$$181 \times 0.058 \times 0.005 \times 100^2 = 524 \text{ lbs.},$$

as before ; so that it would require twice the power, or 140 H.P. These figures do not appear favorable for this mode of navigation. The power diminishes with the angle and load to be supported. Thus, if the load were 1,000 lbs. and the angle of slope be 5°, then at a velocity of 50 miles per hour the area of the plane would need be only 120 sq. ft. and the power 12 H.P., and for 100 miles per hour, 96 H.P.

Langley's experiments give, for small angles,

$$N = \varphi S v^2 i, \tag{14}$$

in which φ is a factor to be determined by experiment. If φ were constant, then, according to this formula, the normal pressure would increase directly as the angle of slope. This is practically the case for angles under 20°, and for that range agrees fairly with Duchemin's. The following are some of the values :

TABLE IV.

DEGREES OF SLOPE.	Ratio of normal pressure upon a plane sloped at an angle i to the direction of motion to that at 90°.	
	Langley's experiments. $\frac{N}{PS} = \frac{\phi v^2 i}{P}$*	Duchemin's formula, Eq. (13). $\frac{N}{PS} = \frac{2 \sin. i}{1 + \sin.^2 i}$
5°	0.15	0.173
10°	0.30	0.337
15°	0.46	0.486
20°	0.60	0.618
25°	0.71	0.718
30°	0.78	0.800
35°	0.84	0.867
40°	0.89	0.910

Commandant Renard used

$$N = P S [a \sin. i - (a - 1) \sin.^2 i],$$

* If ϕ were the same for a plane at right angles with the line of motion as for a plane inclined, we would have ;

$$\frac{N}{PS} = \frac{\frac{1}{2} \phi v^2 \cdot 2 i}{P = \frac{1}{2} \phi v^2} = 2 i.$$

But, according to Langley's experiments, ϕ is less for small slopes than for large ones.

in which $a = 2$. This was probably deduced from Duchemin's, as it results from the latter by developing to two terms, and adds nothing to our knowledge of the subject. In regard to the normal pressure upon small planes inclined at a small angle to the direction of motion, the law may be considered as well established. We know of no experiments upon very large planes moving at high velocities, nor upon large curved surfaces. The chief value in determining the normal pressure is to proportion the plane and not to determine the required propelling power.

The following conclusions may be drawn:

1. The resistance to propulsion will be less the smaller the angle of slope.
2. For a given slope the resistance to horizontal propulsion will vary directly, and for small angles of slope very nearly as the normal pressure, whatever be the law determining the normal pressure.
3. The less the slope the greater must be the velocity in order to support a given weight.
4. The propelling power will vary nearly as the product of the supporting capacity, the velocity and angle of slope.

Other questions than those of speed and supporting power must be considered, such as the difficulty of rising from the earth at any place, the means of safely alighting, the balancing of large planes, especially in the presence of conflicting currents, and the danger of capsizing when run at high speeds at low angles of inclination, especially in crossing counter currents. These have all been considered, but none, so far as known, have been satisfactorily worked out. Experiments on a large scale are necessary in order to determine the real nature of the practical difficulties to be overcome, as well as the best means of overcoming them. It appears to the writer that the difficulties to be overcome are too numerous and the necessary expense too great to lead one to be confident of successful aviation by this method in the near future. While saying this, we are not ignorant of the recent success in mechanical soaring for a few hundred feet.

We now consider

FLOTATION.

In this system the weight is supported by an immense "gas bag," and propulsion produced by a propeller. I prefer the term "gas bag" to that of "balloon," for the latter is associated with a spherical vessel *floating* in the air; and as a further distinction I suggest that the entire plant of a directable balloon—the gas bag, car, suspenders, motor and propeller—be called an "air ship."

The form of the gas bag should be such as to offer the least resistance to forward movement in air Newton's problem of "the body of least resistance" moving in a fluid is not applicable to this case. That eminent philosopher considered only the resistance due to normal pressures against the forward end

of the body. He did not consider the effect of pressure against the rear end as the fluid rushes back, after displacement, against the sloping sides of the body, nor the friction of the fluid along the sides of the body. Langley's experiments show that the friction of the air or "skin resistance" is so small it may be neglected; and, as has been shown, Newton's law of normal pressure is so erroneous that this solution is of no value for this case.

The theory of steamship construction may give some information in regard to the proper form. The resistances here considered are skin friction and the formation of waves and eddies. The former, for moderate velocities, varies, according to Hagen, as the first power of the velocity ("Mechanics of the Earth's Atmosphere," by Cleveland Abbé, p. 25). For higher velocities and greater speed much is left to be desired (*ibid.*, p. 7). It is generally assumed to vary nearly as the square of the velocity, but its value for a bag covered with netting is not known. As before remarked, the experiments of Langley indicate that this resistance is so small it may be neglected. The formation of waves and eddies is a continual draft upon the energy of the motor, since they cause a motion of the fluid, the energy of which is not restored to the ship. At low speeds only small waves and eddies are formed, and the chief resistance is the skin friction; the mass of water, pushed to the right and left by the fore part of the ship, rushes against the aft part as it returns to fill the space which would otherwise be left by the advance of the ship.

The investigations of Mr. R. E. Froude, published in Vol. XXII of "Transactions of Naval Architects," laid a foundation for proportioning ships. The idea was advanced that the relation between the length and speed of a ship had much to do with the resistance at that speed. In this theory it was conceived that a set of waves transverse to the ship is formed by the entrance, and another set by the run or rear end of the ship; that these waves have a length from crest to crest, depending upon the speed of the boat. If the speed is such that the crest of the bow waves fall into the troughs of the stern waves, little energy will be lost in waves; but if the crest of the bow waves fall upon the crest of the stern waves, the loss from this cause will be a maximum.

Mr. J. Scott Russell was led to the conclusion that the water in front of a ship in motion was carried away by a solitary "wave of translation" of definite length, while the space at the stern is filled by a rolling wave, the speed being equal to that of the ship. From this it was concluded that the best length of the entrance part of a ship is the length of the wave of translation, and the length of the run (or quitting part) should be half the length of the rolling wave traveling at the same speed. He also concluded that the form of the entrance lines should be curves of versed sines, and for the quitting lines, trochoids. The *Great Eastern* was constructed on this theory. It is known as the "wave-line theory." A modification of this theory, and known as the "wave-form theory,"

consists in making the areas of the successive transverse sections follow the laws of the versed sinusoid and the trochoid.

Professor Rankine investigated mathematically the flow of water around a body submerged in it, and his theory is known as the "stream-line theory." The form of the body has no influence on the resistance aside from friction and the formation of eddies. The investigation was made for a ship entirely under water, and hence should be directly applicable, if at all, to the balloon part of the air ship. The resistance is found by first finding the so-called "augmented surface." By this system the lines of bow are much blunter than by the wave-line theory, and those of the stern are finer, and hence follows the general features of the fish; but, on the other hand, the entire length of the stern lines should be about $\frac{2}{3}$ that of the bow lines, which is contrary to the form of the fast-swimming fish. Between the bow and stern lines the body may be nearly straight and of suitable length to make the resultant waves a minimum.

Observations are not easily made in air for determining the length of waves and the eddies produced, hence we at present get little if any aid from these theories. Indeed, it seems improbable that "waves of translation" should exist to a perceptible degree; and if not, it would seem that the bow and stern lines should be about equal in length, resulting in a cigar-shaped form. If our reasoning is correct, the form adopted by MM. Renard and Krebs, in *La France*, was approximately the best, but may be further improved. M. Cazin has recently published a theory and formula for the resistance to a ship's motion (*Journal Franklin Institute*, 1893, March, April and May). The simple use of a mean area, or, rather, a section equivalent to the volume of displacement divided by the length, regardless of the form of the water lines or of the nature of the surface, does not commend itself to the writer, to say nothing of the theory of the manner in which the liquid is supposed to move in filling the space previously occupied by the ship.

I have sought the form of the surface of least resistance, assuming a constant volume and length and Duchemin's law of resistance. We would have:

$$\textit{Volume} = \int_0^l y\,ds. = \text{constant},$$

$$dS = 2\,\pi\,y\,ds.$$

$$\text{Horizontal resistance} = 4\,\pi \int_{s_0}^{s} y \frac{\dfrac{dy^2}{ds^2}}{1+\dfrac{dy^2}{ds^2}}\,ds,$$

$$\text{and} \int \left(y \frac{y'^2}{1 + y'^2} + a\,y \right) d\,s = \int V\,d\,x,$$

to be minimized. In this $y' = \frac{d\,y}{d\,s}$. This was solved for the writer by Mr. Joseph G. C. Cottier, a student in the senior class of Stevens Institute of Technology. With the aid of Carll's "Calculus of Variations," he found the form to be an hypocycloid. This, however, proves to be of little or no service, for the curve has a cusp before reaching midship, and hence fails to make a proper curve from stem to stern; and worse than this, Duchemin's formula being for a plane, includes the effect of rarefaction, eddies, etc., on the rear side of the plane, while in a properly constructed air ship no such action exists on the rear side of the surface.

There appears to be no exact theory for determining the best form of the gas bag, and one is left at present only to suggestions of a general character. The ratio of the length of keel to the beam and of the lines of a ship have been established, if indeed they can be said to be *established*, only after long experience; and it will probably be the same with air ships.

The most remarkable air ship up to the present date was made by Messrs. Renard & Krebs, and was named *La France*. The gas bag was $27\frac{1}{2}$ ft., in diameter at its largest cross section, 165 ft. long, held 65,886 cub. ft. of hydrogen, lifting power of 4,402 lbs.; car, 105 ft. long; diameter of its two-arm propeller screw, 23 ft., driven by storage batteries whose power was determined by experiment to be 9 H.P., the motor weighing 180 lbs. per H.P. Several trips were made in 1884 and 1885 from Challais out and back, as follows:

M. R. Soreau, in *Ingénieurs Civis*, February, 1893, gives a table of the trips made by *La France* (Renard & Krebs's ship), which I have arranged in the order of speeds, and deduced the numbers in the fifth column, as follows:

TABLE V

No. of Trial.	Date.	Revolution of Propeller per Minute.	Speed Miles per Hour.	Speed in Miles per Hour for one Revolution per minute of the Propeller.
4	Nov. 8, 1884 ...	35	8.95	0.256
1	Aug. 9, 1884....	42	10.74	0.256
2	Sept. 12, 1884...	50	12.74	0.255
3	Nov. 8, 1884 ...	55	14.08	0 256
5	Aug. 25, 1885...	55	14.08	0.256
6	Sept. 22, 1885...	55	14.08	0.256
7	Sept. 23, 1885...	57	14.53	0.255

From this it appears that the speed of the ship was almost exactly proportional to the number of revolutions of the propeller, so that the per cent. of slip was practically constant. The results are remarkable for their uniformity. According to this, in order to drive the *La France* 29 miles per hour, or double the greatest speed, the propeller must make 114 turns per minute. Since the resistance varies as the cube of the velocity, it would require 72 H.P. to drive it 29 miles per hour, provided the full 9 H.P. was developed at 14½ miles. With the same floating capacity this would require that the weight of the motors, including their necessary appendages, should not exceed 15 lbs. per H.P.; but the means of accomplishing this are not "in sight." If a H.P. were realized with 65 lbs. of dead weight, about 18.8 miles could be realized. At 114 turns per minute the circumference of the propeller wheel would travel 114 × 3.1416 × 23 = 8,240 ft. per minute, or more than a mile and a half per minute. These figures impress one with the great difficulties to be overcome in realizing high speeds with this mode of navigation.

Assuming that the resistance of the air varies as the area of the largest transverse section A, and as the square of the velocity, we have:

$$\text{Resistance, } R = b\,A\,v^2 \text{ lbs.},$$

where b is a constant to be determined. If c be the velocity in miles per hour, then will the work be:

$$\begin{aligned} U &= b\,A\,c^3 \text{ pound-miles per hour,} \\ &= b\,A\,c^3 \times \frac{5280}{3600} \text{ foot-pounds per second,} \\ &= \frac{22}{15}\,b\,A\,c^3. \end{aligned}$$

If the full power of the batteries, 9 H.P., were exerted in producing 14½ miles per hour, then we find for *La France* that $b = 0.0019$; hence

$$U = 0\ 0028\,A\,c^3 \text{ foot-pounds per second,} \qquad (15)$$
$$R = 0.0019\,A\,c^2 \text{ pounds.} \qquad (16)$$

The efficiency of the shaft and propeller was assumed to be about 50 per cent. of the power transmitted to the shaft, hence the resistance of the gas bag, car, suspenders, etc., would be:

$$R' = 0.0009\,A\,c^2. \qquad (17)$$

Comparing this with equation (6), it appears that the resistance of the air ship is $9 \div 50 = \frac{1}{5.5}$ that of a plane surface equal to the largest transverse section of the gas bag, normally exposed. This resistance must be divided between the gas

bag and the car and other surfaces, but no data is given for determining these respective values. Dupuy de Lôme *estimated* that, in his air ship, the balloon alone offered a resistance equal to $\frac{1}{30}$ of that of a normally exposed plane equal in area to its largest cross section. If this were the correct value for *La France*, then would the appendices resist about $5\frac{1}{2}$ times that of the balloon. This result seems improbable ; but whatever its value, the resistance of the attachments must be nearly the same for the gas bag as for the aeroplane. The total resistance, including the propeller and shaft, would be about $\frac{2}{5}$ that of the largest section normally exposed. About the only commendable feature of the aeroplane appears to be the small relative resistance offered to propulsion at small angles of slope. Thus, according to Table III, the resistance at a slope of 5° is $0.015 = \frac{1}{66}$ of that of a normally exposed surface. This assumes that the aeroplane is a perfect plane ; and to this must be added the resistances offered by the attachments, propeller, etc., and these, apparently, would form the greater part of the total resistance. The largest cross section of *La France* was about 550 sq. ft., and a plane at an angle of 5° slope at $14\frac{1}{2}$ miles per hour would support only 100 lbs. Indeed, an aeroplane that would support 4,400 lbs. (as did *La France*) at a velocity of $14\frac{1}{2}$ miles per hour at a 5° slope would offer a greater resistance than *La France*. For, according to Table III, the horizontal force is about $\frac{1}{12}$ the vertical for this slope, and hence must be $\frac{1}{12}$ of $4,400 = 350$ lbs. ; and equation (16) gives for *La France* 220 lbs nearly. The latter 220 lbs. includes all the resistances, while the former 350 lbs. is the resistance of the plane only. The area of the aeroplane for this case must be about 26,000 sq. ft. At a less slope the resistance and supporting power would be less, and at a greater angle both would be greater ; still this example shows sufficiently that for low velocities the aeroplane cannot compete with the gas-bag system. Great anticipations have been raised by the soaring experiments of Lilienthal, but until he has produced something that will sustain a man longer in the air and be more manageable than those shown by his best efforts, nothing can be safely predicted in regard to the final results.

The following are some of the advantages of the flotation system over aviation :

1. It may rise at any place without special mechanism.
2. It may land with comparative safety.
3. It is comparatively stable, and hence not so liable to capsize.
4. The supporting power is independent of the propelling power, and hence is safer and more easily managed.
5. One great advantage in the argument at the present time is the fact that a navigable ship of the flotation system has produced all that was expected of it except speed, which is more than can be said of the aviation system. But the practical problem from a commercial standpoint will ever be menaced with the facts that the ship must be large and yet comparatively light and hence comparatively weak. Considering the many questions requiring solution and the cost of experi-

menting, I anticipate that progress in attaining 25 or 30 miles per hour will be comparatively slow. I am aware that it is claimed that this speed is already "in sight;" but the above investigation leads the writer to infer that it may be several years before it actually comes within the range of vision of "short-sighted" persons. The mechanical means by which it may be secured are well understood, but practical difficulties are certain to be developed at every step in the progress. If a motor and its supplies were furnished at 20 lbs. per H.P., and 60 such H.P. manned *La France*, weaknesses would certainly be developed. If one could secure strength regardless of weight, as may be done in steamships without prejudice, such difficulties would be at once overcome; but such is not the case—weight is an enemy to the air ship. A speed of 30 miles per hour produces relatively a brisk breeze, and the effect on the framework might be injurious; and to talk of carrying a man 100 miles per hour in an air-ship we consider rather "flighty."

MANŒUVRING OF BALLOONS.

By Carl E. Myers, Aeronautical Engineer, Balloon Farm, Frankfort, N. Y., U. S. A.

NOVEL BALLOON EXPERIMENTS.

Celerity of Movement in Gas Vessels.

The chief object of this paper is to induce discussion, and to this end I shall present the circumstances attending various experiments of novel or unique character, some of which will probably conflict with accepted ideas on the subjects involved. In this connection, let me say that "events which, according to generally received opinions, ought not to happen, are often the key to knowledge!"

For brevity I shall only outline my researches in several fields by recounting typical experiments which seem to be of unusual or useful character.

The first of these relates to the swift passage of variously shaped bodies through the air. I had been building balloons and operating gas ascensions by the aid of my aeronauts some two years before I made my first ascension. Meanwhile, I had evolved some theories, and the purpose of my first flight was to determine whether or not a bag of gas of comparatively slim proportions and flimsy envelope could be urged swiftly through the air with moderate power and preserve its integrity and its form within reasonable limits by means of gas pressure within and suitable netting without; as I could not at that time conceive of any material of equal weight capable of form-

ing so strong an internal bracing as hydrogen gas or any means of restraint equal to a close-fitting net.

The balloon was of about 15,000 cub. ft. capacity, formed of gores calculated for a pear-shaped balloon of 32½ ft. diameter, or 18,000 cub. ft. capacity, but using only 28 segments instead of 32, of machine varnished balloon fabric of 39½ in. width. Half full, this balloon contained 7,500 cub. ft. of coal gas, supporting 300 lbs. weight, consisting of the balloon envelope, valve, net and concentrating ring, amounting to 159 lbs.; myself, 115 lbs.; 25 lbs. ballast (besides a fine aneroid barometer, knife, note-book, compass and watch, all weighing less than a pound). No basket or car was attached, but I sat simply astride of a folded band of cloth hung like a saddle from the concentrating ring. Thus my feet extended below, while the balloon neck, hanging down beside me, made us all one machine for cleaving the air.

The air was calm and the ascent slow and nearly perpendicular, reaching finally to two miles in height. To avoid falling off I tied the free ends of the netting cords about my waist, securing myself to the ring. The balloon neck still hung limp by my side, and this also I secured. Then, noting time and altitude, I pulled the valve wide open. The balloon responded immediately, and the speed constantly accelerated till the first downward mile was made in one minute. The neck of the balloon formed a sharp prow, terminating with my body and extended toes. Retaining the slim form throughout, my craft cleft the air like a dart, following any direction in which my lower extremities pointed, and I found I was steering the whole from the prow somewhat as a bird gives direction by its extended beak. I estimated the speed now to be more than a mile per minute, but as it was more or less slantwise, according to the inclination or direction I gave it by my movements, I could only judge by the sting of the air rushing past me. I was now within 1,000 ft. of the earth two minutes after starting. I dropped half my ballast and released the balloon neck. The speed was checked, and as the balloon neck flew up, forming a concavity in the netting above me, there was a shock which seemed to lift me upward. Then the neck fell again, the netting became slim as before, and the balloon slid downward into a cornfield without shock, and ended my experiment.

As nearly as I could figure it out, a slim vessel, approximately wave line in form, of about 7,500 cub. ft. "tonnage," and about 65 ft. in length, including myself, had been driven through the air at the rate of a mile per minute as a minimum speed, with no more than 12½ lbs. gravitative power, guided from the prow with ease, and had sustained no harm or great deformation, though a frail and bulky vessel braced by gas and harnessed with netting. I concluded that the form itself gave it such advantage that it not only met little resistance to its passage, but that it also had little tendency to change its shape.

I was fascinated with my experience, and strong with desire

to go aloft again for further experiments. These subsequently satisfied me that gas navigation must form the basis of earlier navigation of the air, and that two features only were of chief importance—a wave-line form of body and a motive power of light weight. On this basis I began experiments in horizontal movement, the fruits of which, embracing several years' experience, I cannot give briefly here, but content myself with saying that, comparing the possible air ship with the possible flying machine in the "present state of the art," there is more *safe* experience obtainable at less expense with a suitably shaped hydrogen vessel than with any form of aviator, and that experiments in performing evolutions and in air current navigation with gas vessels will do more practical service for aviation itself than any actual aviation of to-day. Furthermore, that any motive power of sufficiently light weight and force to render the aviator practicable will be equally of use or of more value if applied to the propulsion of suitably shaped gas vessels in the immediate future.

Movements of the Spherical Balloon and the Parachute.

As illustrating the contrasting features of the elongated balloon compared with the globular balloon in celerity of movement, I will narrate an experience when the conditions varied.

My balloon was one of 22 segments, each 39½ in. in greatest width. It weighed 80 lbs. complete and was plump full of hydrogen, say about 5,500 cub. ft. Suspended below the car was a parachute formed of a half sphere 24 ft. in diameter, with a 10-in. hole in its top. It weighed 25 lbs. and had 100 lbs. of sand attached. My balloon carried in addition 30 lbs. of ballast and instruments and my own weight of 115 lbs., or a total of 350 lbs., including its own weight of 80 lbs.—the hydrogen, manufactured from water with portable apparatus patented by me, having a lifting power of little over 60 lbs. per 1,000 cub. ft.

In still air the balloon arose vertically 1,000 ft. in 10 minutes. At this point I fastened the valve open, and as the barometer almost instantly denoted a descent, I cut the parachute loose. My car consisted of a light, thin board platform hung in hammock netting, a feature patented and used by me habitually as a rudder platform to guide the direction of the flight or fall. Crouched on this I could see, through the open meshes, everything below. The completely closed parachute opened in 1½ seconds, at a distance of about a rod from me, and it apparently stood fixed in the air as I shot up rapidly from it. After the first upward plunge the balloon began an oscillating and spinning motion, making a sinuous or spiral track. The barometer marked 6,000 ft. elevation 30 seconds later, and 10,000 ft. 1½ minutes after cutting loose. At this point I had checked the ascent so that I only arose 2,500 ft. further in the next 30 seconds, or a total of 11,500 ft. in 2 minutes.

The parachute meanwhile appeared vertically below me,

and did not reach earth till 10 minutes after being detached and it landed about 800 ft. from the place of ascent.

Dangers of the Parachute.

I feel called upon here to refer to the deadly character of the parachute as operated of late years as a spectacle in connection with the hot-air balloons, projecting it into the air for a fall with its aeronaut. The word "parachute" conveys to many minds the idea of safety, but in truth there is no aerial apparatus which has proved so destructive to human life or done so much to discourage legitimate ballooning through the horrible fatalities attending the so-called "parachute drop."

These fatalities fill a scrap-book in my collection of news clippings, and would cover an entire newspaper page if collected as a list, covering the simple dates, names, places and brief circumstances. Some five seasons ago I indulged in a series of parachute experiments, consisting of various contrivances dropped from captive balloons at an elevation of 1,200 ft. This was, I believed, the only, or at least the first systematic line of experiments ever conducted with the parachute. Owing to the precaution of beginning all tests by dropping sand bags till perfect conditions of safety survived, no human sacrifices were made, but the percentage of sand bags destroyed was frightful and discouraging. New York State has since set a worthy example in outlawing the spectacle.

SYSTEMATIC EXPLORATIONS OF THE UPPER AIR, WITH ESTIMATES OF COST.

By Mark W. Harrington, Chief of Weather Bureau.

The exploration of the upper air is the immediate requirement for the satisfactory advance of meteorology. There is abundant reason to think that many of the changes which go under the name of weather have their origin at some distance above the earth; and of what occurs in the cloud layer or layers, our knowledge is insignificant or theoretical. The only systematic attempt to investigate the higher atmosphere has been by means of mountain stations; but this, though it has led to a series of interesting results, does not meet the requirements of the meteorologist. The station on the mountain-top is, after all, only a station on the earth's surface; and though many of the equidynamical surfaces show change with the elevation of the land (the isobaric, for instance), others (as the isothermic and those for wind and humidity) show marked adaptation to the contour of the surface. Many aeronauts have noted this adaptation as especially true of the cloud lay-

ers, the lower one often reproducing with some exactness the general variation of the surface below. We can hardly expect, therefore, that the mountain stations, useful as they are, will give us the aid needed in ascertaining what goes on at considerable elevations in the free air.

There are several ways of exploring the upper air by investigating the ray of light which has passed through it. The spectroscope promises much in this direction. The twinkling of the stars might be expected to give us a great deal of information when properly interpreted ; and Sr. Ventosa has shown that even the fluctuations on the margins of the larger celestial bodies, when viewed in the telescope, have apparent relations with the upper winds. This information must, however, be vague, because the total result received by us is the integration of the individual effects at each point of the path, and it is not practicable to separate the sum into its parts. Besides, even if this could be done, the information to be obtained would be very incomplete, as it would relate only to a part of the series of meteorological elements. It may be mentioned as of interest in this connection that the scintillation of stars has been especially and systematically studied ; and M. Dufour, one of the leading students of the subject, has recently announced (" Archives des Sciences Phys. et Nat.," June, 1893) that the only meteorological result he has been able to reach is the rule that lessened twinkling indicates bad weather.

There remain as means of systematic exploration of the free air, elevated towers, kites, pilot balloons (without aeronauts), and balloons carrying aeronauts. The elevated towers are well illustrated by the Eiffel Tower in Paris. By such a tower a systematic study may be made of a layer of air 1,000 ft. thick, with almost infinitesimal perturbations by the tower itself. The excellent series of observations made by the French National Service on the Eiffel Tower have proved of very great interest, yet they do not reach to the height needed for the study of the upper air. It tells us nothing of what happens in the cloud layer, probably the most important of the strata of the atmosphere. Moreover, such towers are very expensive to build and to maintain. I have heard the cost of the Eiffel Tower estimated at $1,000,000 and its maintenance must cost a considerable sum, which could only be met by using the tower as a permanent show-place ; the latter requirement necessitates its being placed in or near some great city.

The method by kites has been studied especially by Mr. William A. Eddy, of Bergen Point, N. J., and the data which I give I owe entirely to his kindness. He uses tailless kites, places them in tandem, and recommends that they be flown in groups of three. By such means he has already attained heights of 4,000 to 5,000 ft., and confidently expects to attain 14,000 ft. without serious difficulty. On my request that he estimate the cost of carrying meteorological instruments to this height, he gave me the following estimate, on the basis that the line would average an angle of 45° with the horizon, and would have to be about 23,000 ft. in length.

COST TO CARRY INSTRUMENTS TO 15,000 FT. HEIGHT BY MEANS OF KITES.

		Breaking Strain.		
8 highest kites....	12,000 ft. cable-laid twine ...	250 lbs....	$6.00	
8 next " ...	3,000 " $\frac{3}{16}$ in. rope	540 "	6.00	
8 " " ...	3,000 " $\frac{1}{4}$ " "	1,280 "	12.20	
9 " " ...	5,000 " $\frac{3}{8}$ " "	2,250 " ...	30.50	
	23,000 ft.			$54.70
Kites....	8 kites 5 ft. across..........	....	16.00	
	8 " 6 " "	....	20.00	
	8 " 8 " "	....	24.00	
	9 " 9 " "	....	31.00	
	33			$91.00
	Windlass for winding kites.	....	20.00	
	Four laborers at $1.50 per diem........	....	6.00	
				$26.00
		Total......	......	$171.70

Mr. Eddy adds "that in lighter winds perhaps 50 kites would be required, the above estimate applying for winds of about 10 miles per hour. All the kites are tailless, and fly at an angle of about 80° from the horizontal for the first 800 ft. of line out. In case the pull becomes too great for the breaking strain, the low and larger tandem kites can be hauled in. The breaking strain of the cordage must be known and the pull at the earth's surface constantly measured to prevent the entire line from breaking away. This is a rough estimate, but is founded upon careful experiments during two years. The top kites and twine should be laid out the night or day before, and the line should be extended along the ground for several thousand feet. Soon after daybreak the top kites should be started up, the top one lifting the next, and so on. The kites will right themselves, regardless of position in which they are when lifted by the higher kites. Instruments should be suspended between two groups of three kites each, thus: (see fig.)

"Three tailless kites will fly when any one of the three will not, in very mild surface winds. For safety it would be well to have the kites in groups of threes."

Mr. Eddy is not ready to give a limit to which kites can be flown, but is not without hope that they can be made to reach the cirrus clouds. In winds of high velocity the kites must be perforated to relieve them from too strong air pressure. The tailless kites easily right themselves when reversed, and a tandem series of kites tends to prevent the jerking which might put the instruments out of order.

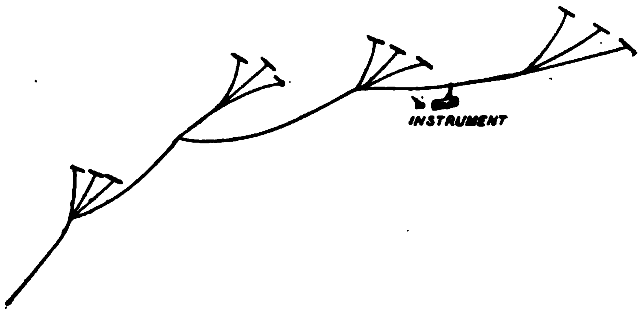
It seems, therefore, that for about $200 we may hope, by means of kites, to take instruments for registry to the height of about 14,000 ft. If we assume a loss and wear of 5 per cent. per day in the kites and other apparatus, we would have a current expense of $10 per day. Add to this the $6 for labor, and such a

service would cost about $16 per day. This would make for current expenses for a year $5,840; add cost of instruments, $2,500; outfit and incidentals, $1,660. Total, $10,000.

A probable total cost, then, of $10,000 for a year's systematic work of this sort, not including the salary of the officer in charge.

PILOT BALLOONS.

The best possible anemometer is a balloon which is immersed in the air and moves freely with it. For half a cen-



tury or more occasional studies of the lower air currents have been made by means of small pilot balloons. The balloon is allowed to rise freely, and a card is attached to be returned by the finder with name, date and place. The most elaborate series of observations of this sort known to me are those of M. Louis Bonvallet, at Amiens, who, from May, 1888, to the end of 1890, sent up 97 paper balloons, varying in volume from 50 to 1,800 liters. The general results are given by M. Gaston Tissandier in *La Nature*, 1891-1892, pages 250, 260. The amount of instruction from them is small and disappointing.

Such balloons can be used only for the study of air currents, but by a proper selection of places and dates, and the assistance temporarily of theodolites and persons capable of using them, these balloons could be made very useful. They would enable us to study the arrangement of air currents about definite meteorological phenomena, such as centers of high or low pressure. To effect this the observer should have a supply of small balloons on hand, and the means of readily inflating them. He should also have on ready call two theodolites and persons capable of using them—a requirement easily filled at any college or university with a department of civil engineering. The weather map should be carefully studied from day to day, and when a "high" or "low" is about to pass over the station, the force of observers should be called out and the balloons inflated and released to as great a number as could be observed at frequent intervals (once in five minutes, say), and with approximate precision. Computation would then easily

show the horizontal motion at determinate elevations below the cloud layer, and plotting would show the relation of these to the center of pressure. Aside from the salary of the principal observer, the cost of such observations would be small for each opportunity to observe, and for any given station the number of opportunities during the year would be few. The expense at any station for a year would probably not surpass $150, so that for $3,000 such observations could be scattered at 20 colleges over the States, with probable results far in excess in value over the cost.

A more instructive but more expensive method is that of pilot balloons carrying automatic registering instruments. This method of sounding the upper air was proposed by Le Verrier in 1784, and has within the last few years been repeatedly tried in France. In the last four months of 1892 M. Hermite sent up 13 such balloons, all of which reached an altitude of over 9 kilometers, or 6¼ miles; and one sent up on March 21, 1893, must have reached an elevation of over 16 kilometers, or 10 miles. These balloons carry means for the automatic record of pressure and temperature, but the last mentioned found so cold temperatures that for a considerable time the specially prepared ink could not perform its functions. They also carried a device for releasing and dropping cards, to enable the following of the course of the balloon; but this has not been successful, as the fuse which releases the cards is soon extinguished. In the ascent of March 21, out of 600 cards taken up, only 400 were released, and of these only five or six were recovered. It is found, however, that the recovery of the balloon is much easier than had been expected, as a printed direction on the balloon itself leads to its recovery as soon as it falls into the hands of any intelligent person.

The difficulties in the way of these remarkably interesting explorations prove to be less than could have been expected; but there are many questions about them still unsettled. Under these circumstances it is not easy to make an estimate of the cost of systematic work in this direction. I have, however, asked Professor H. A. Hazen to make the estimates for me. He has estimated approximately that a balloon to ascend to a height of 4 miles with a load of instruments of 20 lbs. would cost $150 if made of silk, and $200 if made of goldbeater's skin. For a balloon to ascend to the height of 10 miles he puts the corresponding prices at $600 and $800. The instrumental outfit would have to be prepared expressly, and would be expensive. Probably the sum of $5,000 would permit of one such pilot balloon per week during the year to the height of 4 miles, and perhaps one per month to the greater height. The station selected for such observation should be near the middle of the continental area—say somewhere from Kansas to Manitoba.

BALLOONS WITH AERONAUTS.

The preceding methods, while they would give highly interesting and instructive results, are somewhat imperfect as

means of obtaining all the information needed by meteorologists. Much better for this purpose would be systematic work by a meteorologist who would make the ascension himself. Evidence points to the conclusion that the cloud layer and perhaps the upper cloud surface is a region of especial activity in meteorological phenomena, but the facts on which such a conclusion could be verified are of such character that they would probably escape any automatic registry. Their rocord requires the presence of a trained meteorologist. These observations should be systematic, as the sporadic ones are of relatively little value. A meteorologist should ascend twice a day to a considerable height, and should keep this up through all kinds of weather and through the season. The elevation need not be great; probably the first 20,000 ft. include the layer of air in which the meteorological phenomena which we call weather are active. At least the stratum of this thickness is far more important to us than all the rest of the depth of the atmosphere.

The cost of such a campaign would be considerable, but would vary with the material used, the care in using it, the position of the station, etc. I think a year's campaign of this sort could be gone through for an expense of $20,000.

In conclusion, it appears that a year's campaign could be made in the free air as follows:

To 8,000 ft. (perhaps) with small balloons	....	$3,000
" 14,000 " " kites	....	10,000
" 20,000 " 52 pilot balloons } " 50,000 " 12 " " }	....	3,000
" 20,000 " with aeronaut.	....	20,000

The results to be obtained would be cheap at any of these prices, but the fourth method seems to me incomparably the best as well as the most certain. A year's campaign of this sort would add very greatly—more than in any other possible way in the same time—to the knowledge of meteorology and hence to the forecasting of the weather. There is no other way, I believe, in which this sum of money could be expended to the greater advantage of meteorology.

[NOTE.—Upon the reading of the above paper it was, upon motion of Mr. D. Torrey, unanimously resolved

"That it is the sense of this meeting that the experiments proposed by Mr. Harrington are likely to prove of public value in forecasting the weather, and that Congress should, in our judgment, make the necessary appropriation to have the experiments made as recommended by Mr. Harrington."]

OBSERVATIONS IN BALLOONS.

By C. C. Coe.

So far as I know, I am now the oldest aeronaut in the United States, having made my *début* in April, 1859.

During my long career I have met with many adventures

and experiences, and I have made many observations of natural phenomena while in the air; but upon the present occasion I desire to refer to but two of them, which may perhaps interest both aeronauts and meteorological savants.

These two observations, or, rather, questions, relate first to the possibility of utilizing balloons in the detection of obstacles to navigation, such as submerged reefs, wrecks, etc., and secondly, to the question whether the upper surface of storms high in air follows the undulations of the ground.

It may not generally be known that when the aeronaut is high in the air, his vision penetrates in clear water to very much greater depths than when near the surface. The intervening space seems to act like a water telescope, and to enable the eye to reach down many fathoms into the water; and as the principal object of submarine exploration in the main is to detect dangerous places, within moderate depths, observations from balloons may become more effective, as well as quicker and cheaper than the running of lines of soundings.

In one of my ascensions from Woodward's Gardens, San Francisco, Cal., I hung over the bay for a long time. Distinctly outlined on the bottom of the bay there was an old hulk. The Government officers knew nothing of it; the oldest seamen there never had heard of it.

In 1871, in an ascension from Oswego, N. Y., I first passed over the Ontario Lake north about three miles; thence, floating east or northeast over Pulaski, Martinsburg, to Watson in Lewis County. While over Lake Ontario we could see the outlines of a ravine in the bed of the lake extending for miles on our way. Its sides looked like the sandstone bluffs on the lake shore west of Oswego.

In this ravine there were a multitude of large boulders. The oldest sea captains of Oswego never had heard or knew of its existence.

These experiences indicate that when the Government does its sea soundings on philosophical principles, more work and better work may be done with a balloon in one calm day than in a whole season of soundings with the same number of hands. Indeed, I feel safe in saying that it could be done at 10 per cent. of the present expense, and with far less labor.

The shoal over which the breakers were seen by moonlight in mid Atlantic about a year ago could have been detected better from a height of 3 or 5 miles from a balloon than from as many rods away on board ship, or found by the means in present use, for the aeronaut can distinguish general outlines in moderate depths of water. You may ask, "What is a moderate depth of water?" I would say, under favorable circumstances, 200 ft. It would depend upon the degree of its transparency. You may also ask, "How do you account for the better vision at a higher altitude?" The answer is, in the same way that I account for the power of seeing stars from the bottom of a deep well at midday. Overpowering reflections and abrupt refractions of light are all so dispersed that

the vision is not overpowered, as the "bull's eye" held upon the victim blinds him.

I trust I have made it plain that a sunken ship, a sand-bar, a reef, a hidden coral bed, or a rocky section can all be best seen from a high altitude, and that not a foot of the neighboring grounds will be missed or neglected. Balloons used for such a purpose should, of course, be of special design for that purpose.

Now, as to the question of the upper boundaries of storms, and whether those great atmospheric waves follow the contour of the ground over which they roll and lie level over water, I may relate one of my experiences.

But first I want to say a few words concerning the theory of there being a permanent eastward current in the upper air in our latitude, of which theory Mr. John Wise, the aeronaut, was the advocate.

All my experience goes to show that the statement is not correct, and that an eastern current is not permanent. It is so temporary, even in its season, that a heavy storm will not leave a vestige of an eastward current in its path. It may afterward regain its course, but it is then entirely bereft of its permanency.

In 1871, on the 4th of July, I made an ascension from Oswego, N. Y. The intention was to take up six passengers—two aldermen, two reporters, one editor, and a doctor. I was advertised to leave at "two P.M." The lieutenant of the Signal Service came to me at 1.30 P.M. and said, "There was a heavy storm raging down the lake. I must get off as soon as possible or let off the gas." The wind led direct over the lake. Not a passenger was to be seen, so I loaded up the car with sand-bags. My helper from Syracuse, in linen clothes, begged the privilege of going; he went. Though it seemed awfully black in the south and west, we began to ascend just as the first eddying gust struck the park thronged with people from whence we started. The gust snatched us like a toy through a tree-top, and in five minutes we were in snow clouds. Just as we entered the clouds we saw a feeble flash of lightning; that was the only flash of lightning we saw, but the thunder was almost incessant and often directly beneath us. We passed through a snow-storm which so weighted the balloon that we halted in the ascent; as soon as possible a 50-lb. bag was emptied, and in a few minutes we were in sunshine, and had risen above the storm in 25 minutes from land. Just as we emerged from the snow-storm a beautiful mellow light greeted us. We had passed through a few feet of dense fog into the most beautiful sight that man ever beheld. There was beneath us a sea of down (in appearance) spreading on every side as far as the eye could reach, and it was level as a house floor. All along the south and west the light was too bright for the eye's endurance. We were so nicely poised that the car seemed to hang nested in this halo of glory. We went above it a few feet, then sank into its bosom again, while we knew that it was but a few feet down to snow; but the second time

we came to the top so nearly poised, we went about 100 ft. above this dazzling plain.

We were doomed not to dwell long in this ideal spot. In the south and west there was a high wall of thunder heads approaching rapidly; beneath us lightning's artillery was sounding in every direction, while the thunder heads, towering like a cliff over the plain, were advancing at a rapid rate as they marched upon us, towering so high and so vertically that we had to look quite close along the balloon in order to see their tops.

Then, level with ourselves, we observed a great dark cavern, about half a mile in depth, yawning in front of us. Its entrance was within a few feet, and its arches towered hundreds of feet above the balloon, while we eagerly scanned the scene and wondered at the strange outlines and minor arcades of this dark and stupendous apartment. While the balloon was on tiptoe to touch the top of the entrance, and we were craning our necks to see the beauties of the home of a storm, we were engulfed in the twinkling of an eye.

The next moment the lower part of the balloon whipped and snapped about like a flag or tattered sail in a gale. We spun around and reeled—all about us was dismal roar; the neck and bottom of the balloon collapsed and became concave, re-entering into the balloon. The rain, snow and sleet pelted us from every direction, coming up and down and crossways in the same minute, if not at the same instant. The accompanying upward rush took us 200 ft. higher, and left us whirling in the rain. Just then, a little one side and below us, a railroad whistle broke the monotony of the thunder, and the next minute it sounded directly under us. This occurred just 40 minutes from the time we started, and the next whistle sounded still farther on the other side than the first. We subsequently found that we must then have been over the city of Kingston, in the province of Ontario, having been blown due north over the lake a distance of 60 miles in 40 minutes.

Meanwhile, the thunder was fearful on every side It was not the crashing, startling sound which we sometimes experience on the earth, but an incessant roar, seeming to come from everywhere. We were in a drenching rain which streamed from the netting and poured down from every side and ladened the balloon.

Presently the gyrating ceased, and we found we were descending rapidly, having fallen from a height of 17,500 ft. to one of 12,500 ft. We emptied four bags of ballast and presently were poised in the air, although still in rain, and found 70 minutes had elapsed since we had started. My passenger in linen clothes, with chattering teeth, sat blanched, chilled to helplessness, his limbs and person wrapped in sand-bags. It was so dark that we could hardly see, but after awhile the thunder became less and less frequent, and soon it died away in mutterings. Then we ventured upon a descent, being still in a heavy rain. When low enough to spy land we found that we were going at a tremendous rate. Just ahead on our path

there was a clearing which was all stump lot. I threw the anchor for a log fence, but the top rail was only a toy for the anchor, being hurled many feet away. This being missed, two prongs of the anchor next took hold in the root of a stump; it held fast, and the balloon swayed over to the ground and instantly was split into four quarters.

It was than 8.20 P.M., and we had landed 90 miles north of Kingston, in Canada, having made a trip of 150 miles, and come down in the woods 6 miles from a settlement and 13 miles from a telegraph station.

My balloon was so thoroughly drenched by the rain that I was a week drying it sufficiently to warrant shipping it without undue risk of spontaneous combustion.

This was the occasion of the public press referring to me as being "Lost in the Canada woods."

The object of this narrative is this inquiry: Does a storm assume a higher altitude over land than over water? Forty minutes and 60 miles brought us to Kingston (beyond doubt), where we heard the railroad whistles. About that time the towering wall of black thunder heads embraced us The storm—the worst of the season—charmed us with a lovely gentleness for a moment; then it shook us as unmercifully as a lion does its prey.

Now, was that part of the top of the storm which was so lovely and bright and beautiful, and so boundless in appearance, resembling a western plain, thus level because the lake was below? In other words, does a storm conform at its top with the configuration of the earth beneath, thus being level over water and broken and irregular over uneven land? Judging from this experience, I think it may be so.

Heated air is the primary cause of a storm; electricity is the great auxiliary in massing its vapors, or in holding them in molecular suspension. Over a large body of water there must be a correspondingly large body of air at one common temperature; the mass of vapor would therefore be of one common height, but remain transparent so long as held in electrical suspense. It becomes opaque and visible (like the top of this storm) the moment that electrical suspense is changed to electrical deposition. Let me explain what I mean by the latter:

It is surprising how quick a snowflake is manufactured in its native factory. First the nucleus is the size of a pin's head. I know not how long it takes to form that, but from that size to that of a large flake it does not consume more than five seconds. I watched a number of them; they would gyrate in descending with us, falling sometimes faster and sometimes slower, according to the way we were poised, and descending or rising. This I call electrical deposition, whether it is forming a snowflake or a drop of water.

Was the thunder head or higher portion of the storm caused by the heated air, which was more rarefied over land than over water, or by two storms colliding with each other at an angle? The gyrations and terrible writhing of the balloon would indi-

cate the latter. Yet a large mass of superheated air might burst up through the superincumbent layer forming the cold stratum above the heated air, and produce the same phenomenon. In the latter case, it would be a new-born storm added to one already under rage, thus making a double force and acquiring more speed, and the more speed the more destruction.

I may remark in this connection that some years ago areas of considerable size in the settled portions of New York State were partitioned off into something like basins by woods; these basins one after the other would be touched off into thunderstorms by heat and electricity, and these seldom became double or triple headed. But now these partitions have been so chopped off that the storms are double, triple and four headed, and are so powerful that no ordinary construction can stand in their path.

BALLOON METEOROLOGY.

By C. E. Myers.

RAINFALL EXPERIMENTS.

Reverting to the temperature changes in the gaseous contents of balloons, I am of the opinion that either the preconceived notions of meteorologists as to causes of atmospheric changes through expansion or dilation of air or gas " doing work" and producing cold (or heat), and condensation, or moisture, or vapor, will have to be overhauled, altered, or abandoned in part, or else our knowledge of physical laws needs additions which only balloon exploration of the air can supply.

This leads me to recount my early experience with the peculiar experiments of the United States Agricultural Department for inducing rainfall by concussions or explosions in mid air.

The various apparatus used, consisting of hydrogen balloons, generators for producing quantities of oxygen and hydrogen gases in the open air, electrical apparatus, kites, etc., were originally made, combined and operated at the balloon farm, my home at Frankfort, N. Y., as the result of a series of experiments there. The first experiment in any way public occurred at Washington, D. C., but just previous to this there was a private experiment there which was eminently successful in all features, the narration of which will be of novel interest at this late date, as it never has been made public before.

Ordinarily in ballooning it is of first importance that the balloons should not explode. In our operations explosion was the result sought after, and the bulk of the mixed gases, oxygen and hydrogen, exceeded anything previously handled together, or exploded by accident or design, anywhere in the world, I believe.

The oxygen was obtained by passing sheet-iron tubes 30 in. long and 3 in. in diameter charged with the usual mixture of chlorate of potash and black oxide of manganese slowly through the fierce blast of a plumber's gasolene furnace, the contents being decomposed at a red heat at the point covered by the concentrated flame, and each retort thus discharged 30 ft. of oxygen in 30 minutes; which gas, washed with lime and soda, passed directly to the balloon till filled one-third full. The hydrogen was generated by the vitriolic process with my portable system, which supplied double the quantity of gas, filling the balloon plump full. This was 10 ft. in diameter and of 550 cub. ft. in capacity.

A duplex: insulated wire cable, terminating in a small fulminate cartridge inserted in the neck of the balloon, allowed it to rise to about 1,500 ft. elevation, giving me meanwhile a smart electrical shock from the clear sky as my hand touched a bare spot on one of the wire conductors.

This balloon bomb was exploded by a hand dynamo while I lay flat upon my back to watch it through a good binocular. At the moment of discharge the 10-ft. sphere instantly dilated to ten times its former diameter, looking like a gorgeous yellow globe of dazzling fire, like the sun suddenly moved near at hand. This as instantaneously disappeared, leaving an equal space filled with twinkling fragments of the envelope, looking like "star dust" or snowflakes. The spectacle, surpassing any I had ever yet seen, was followed about a second later by the must stunning CRASH I had ever heard. Thunder and the boom of heavy guns seemed surpassed. Following this, in rattling and reverberating succession, came the jarring roar of thunder-like echoes, reflected again and again from earth and air as the shock rolled along the hill-tops and through the vaults of the sky, extending for a period much beyond the ordinary duration of thunder claps.

(Although later, in Texas, balloon bombs of 12 ft. in diameter, containing nearly twice as much of the mixed gases, were exploded, they did not seem to yield anything like the same volume of sound.)

Following the explosion of the 10-ft.-diameter bomb, the roof and walls of the casino, club-house, bowling-alley, situated directly below the point of discharge, spread and then collapsed. Small fish in the brook below were found dead in large numbers. As a coincidence, the skies burst open before we could pick up and store our apparatus, and a heavy shower of nearly an hour's duration chased us into the city from the "boundary" where we had operated, the day having been a fine one, warm and clear previous to the discharge.

I conclude with one account of a balloon bomb explosion reported by one of my operatives, who assisted the last governmental rainfall experiments at San Antonio, Tex., in November, 1892.

This balloon, of 12 ft. diameter, was the last fabric balloon remaining, and had been a long time filled, as it had been used as a gas reservoir and carried about the field. It arose slowly

with a lighted fuse, and exploded at about 400 ft. above a house. It broke windows both inside and out, scattered milk-pans piled on the steps, burst a thermometer bulb and turned its face to the wall, stripped a piano of its spread, and from beneath articles left on the instrument, trimmed all the Spanish moss hanging from the mantelpiece and clock as if cut by a knife, displaced pictures on the wall and upset those on a table, tore open a clasped pipe-case and removed the pipe, broke lamp chimneys and the hall rack mirror, and performed various eccentric pranks. Only a slight report was observed at the time.

SCIENTIFIC RESULTS GAINED BY BALLOONS.

By H. A. Hazen.

In June, 1783, the first balloon ever constructed was sent into the air at Annonay, France, and it landed at a distance of a mile and a half. Shortly after this, Franklin saw a balloon ascension in France, and with his usual sagacity, foresaw and spoke of the great importance of balloon observations to the science of meteorology. In all these years the balloon has afforded amusement to multitudes at *fêtes* and on special occasions. It has, however, been rarely employed in the exploration of the atmosphere, though admirably adapted for that purpose. The first ascension of any value was that of Gay Lussac, in 1804, from Paris. The balloon rose to 23,000 ft. The fall in temperature was to 14.9°, Fahr., or 1° in 340 ft.

Specimens of air collected at the highest point showed precisely the same composition as that at the earth. The magnetic force did not experience any sensible variation at the different heights. Gay Lussac remarked that at the highest point reached there were still clouds above him ; and it should be noted that in no case since has any one ascended above the highest clouds.

The next ascent to be noticed was that of Barral and Bixio, in July, 1850. In this ascent, at 19,700 ft. they observed a temperature in a cloud of 15.0°, and at 23,000 ft., just above the cloud, 38° F. below zero, giving almost the incredible fall of 53° in 3,000 ft. The only balloon ascensions of special value in England were those by James Glaisher between 1862 and 1866. In one of these, Glaisher became unconscious at about 29,000 ft. ; but he probably ascended to 31,000 ft., which is the highest ever attained in the open air. It should be noted that with a balloon of sufficient size we may be enabled to reach very much greater heights without discomfort by using a pneumatic cabinet. Glaisher's results may be summarized as follows : Within 100 ft. of the earth there may be a decline of temperature of several degrees during the midday hours, and an increase under the same conditions at night. This decline varied near the earth with the amount of clouds, moisture,

etc. Within the first 1,000 feet there was an average diminution of 1° for each 228 ft. with a cloudy sky, and for each 162 ft. when the sky was clear. At 10,000 ft. there was a decline of 1° in 455 and 417 ft. respectively. Above 20,000 ft. the decline was 1° per 1,000 ft. These results are of the highest interest, and may serve to explain, in part at least, the mild climate of England. The usual law of decrease has been assumed as 1° in 300 ft.; now, if the air grows relatively warmer as we ascend, it may form a warm cover which will prevent the cooling usually noted at other places at even much lower latitudes. For example, England, though 700 miles farther north then New York, has very nearly the same temperature. Glaisher does not review his hygrometric observations, although, if accurate, they must be regarded as more valuable than the temperature results for most purposes. This omission may have been due in part to the use of a stationary psychrometer, which, it is known, will give very discordant results.

In the United States Professor John Wise made 466 ascensions, and one of these was the longest and perhaps the most memorable on record. In this voyage a balloon of about 67,000 cub. ft. capacity was used, which carried, with 4 passengers, 1,000 lbs. of ballast. The start was from St. Louis on July 1, 1859, and the landing was made 19 hours later in Henderson, N. Y., the distance of 850 miles having been traversed at the rate of 45 miles per hour. A good deal of the distance the balloon was at a height of 10,000 to 12,000 ft. above the earth, and this accounts for the great velocity attained. This voyage gives facts regarding the steadiness and velocity of the movement of the atmosphere over vast regions which had never been imagined even before. Professor Wise was hampered somewhat in his ascensions by the fact that he did not make use of accurate instruments, but he has given descriptions of his experiences in violent storms, showing commotions and disturbances in the atmosphere which are of the greatest interest, and which demand a thorough and painstaking research. One of these will suffice. In an ascension at Carlisle, Pa., June 17, 1843, he entered a thunder cloud without hesitation. At first there was a suffocating sensation. Almost at once the cold became intense. He says:

"Everything around me of a fibrous nature became thickly covered with hoar frost, and the cords running up from my car looked like glass rods, these being glazed with ice and snow. The cloud at this point, which I presumed to be about the midst of it, from the terrible ebullition going on, had not that black appearance I observed on entering it, but was of a light, milky color, and so dense that I could hardly see the balloon, which was 16 ft. above the car. I soon found myself whirling upward with fearful rapidity, the balloon gyrating and the car describing a large circle in the cloud. A noise resembling the rushing of a thousand mill-dams, intermingled with a dismal moaning sound of wind, surrounded me in this terrible flight. I was in hope, when being hurled rapidly upward, that I should escape from the top of the cloud; but the

congenial sunshine, invariably above, which had already been anticipated by its faint glimmer through the top of the cloud, soon vanished with a violent downward surge of the balloon—as it appeared to me—of some hundred feet. The balloon subsided only to be hurled upward again, when, having attained its maximum, it would again sink down with a swinging and fearful velocity, to be carried up again and let fall. This happened eight or ten times, all the time the storm raging with unabated fury, while the discharge of ballast would not let me out at the top of the cloud, nor the discharge of gas out of the bottom of it, though I had expended at least 30 lbs. of the former in the first attempt, and not less than 1,000 cub. ft. of the latter, for the balloon had also become perforated with holes by the icicles that were formed where the melted snow ran on the cords at the point where they diverged from the balloon, and would, by the surging and swinging motion, pierce it through. Once I saw the earth through a chasm in the cloud, but was hurled up once more after that, when, to my great joy, I fell clear out of it. I landed in the midst of a pouring rain 5 miles from Carlisle. The density of this cloud did not appear alike all through it, as I could at times see the balloon very distinctly above me ; also occasionally pieces of paper and whole newspapers, of which a considerable quantity were blown out of my car. I also noticed a violent convolutionary motion or action of the vapor of the cloud going on, and a promiscuous scattering of the hail and snow, as though it were projected from every point of the compass."

Professor Wise repeatedly noted a repulsion and attraction in clouds as well as a violent rotation of the balloon, but he quite definitely settled that there was no swinging of the balloon around the arc of a large circle, as seems to be indicated by some theories.

In the United States there have been very few balloon ascensions for scientific purposes. In 1885 Mr. Hammon made four under the auspices of the Weather Service. These did not attain a very great altitude, but they were the first in which a sling psychrometer was used for determining the temperature and moisture of the air. The present writer has made four voyages, ranging from 7,000 ft. to 16,000 ft. In all of these the sling psychrometer was used. At Philadelphia, on August 13, 1887, the Delaware was crossed at a height of 8,000 ft., and the evidence was strong that the water had no appreciable effect on the moisture of the air at that height. In this voyage a relative humidity of 11 per cent. was found at 7,000 ft. It is probable that no such low humidity as this was observed in the 17 years during which observations were made at Mount Washington. The principal interest in these voyages, however, centers in the discovery of well-defined layers of moisture in the atmosphere, even when there were no clouds. These would seem to indicate a want of homogeneity in a vertical direction, and also a probability that moisture and temperature conditions are to be found distributed along horizontal planes. We may well ask what bearing this distribu-

tion of moisture has upon a storm. How do these planes thicken up as a storm approaches? What part does dust have in their formation?

I have thus given a brief review of the principal points developed in the rather scattered attempts thus far made in exploring the atmosphere. I am sure every one will admit either that this is an inadequate representation, or that very little has been accomplished thus far. There has been no systematic exploration of the air, and yet there cannot be the slightest doubt as to its great importance. A few well-conducted ascensions in rain storms, cold waves, hot waves, etc., will add very materially to our knowledge of weather conditions. If only we could ascertain the cause of these phenomena we could hope thereby to assist all those who are susceptible to the weather and its changes. We would be in much better condition to predict weather changes, and an advancement in this direction will be of great benefit to the farmer and sailor. The expense of such systematic exploration need not be very great if properly managed. It is very much to be hoped that we shall soon have the proper means and a fair fund to carry out such an enterprise, and thus to lay a good foundation in the science of storms and other disturbances in the ocean just above our heads, but in the past so far beyond our reach.

Mention should be made of the fact that excellent photographs have been made of the earth's surface from the balloon; and it is believed by many that the topographer may yet gain very material assistance in his work by means of such photographs, and this too at a greatly reduced expense. Quite recently an enterprise has sprung up in France by which it is hoped to bring down information from heights of 15 or 20 miles. One balloon (*Aerophile*) has already been sent to 10 miles by M. Hermite at Paris. In this case the ink of the self-register froze at a temperature of 51° C. below zero at 46,000 ft.

EXPLORATION OF THE UPPER ATMOSPHERE.

BY N. DE FONVIELLE, PARIS, FRANCE.

IN a recent issue of *Nature* there was an article on the Bombardment of the Sky, giving a *résumé* of the explorations of the upper atmosphere. In accordance with a letter from the Secretary-General of the Congress on Aerial Navigation, I wrote to M. Hermite to learn the result of his experiments, and why they had been apparently discontinued.

He has now several pieces of apparatus which he has had made at his own expense, and with which he hopes to obtain results of such a nature as to attract the attention of the scientific world. It is the delicacy required in the manufacture of these pieces of apparatus that is the sole cause of the delay.

Great honor is due to M. Hermite for his persistency in this

direction, for no one before him had so much as made an attempt to reach these great elevations, being deterred by the fear that it would be impossible to recover the balloons; but the experiments that have been made prove this fear to be unfounded, since, in a civilized country like France, the return is easily secured even from the fastnesses of the mountains, whither one had been driven by the winds.

The following is a detailed account of the experiments of M. Hermite: Since the wonderful ascension of the *Horla* to the height of 23,294 ft. on August 13, 1887, by MM. Jovis and Mallet, no ascension to a great height has been attempted. The exploration of the upper atmosphere, however, is of a scientific interest of the first order, and will go far toward solving a multitude of problems. In order to exceed the limits which have heretofore been attained, the courage of the aeronauts hurls itself against difficulties which are almost impossible to overcome. In fact, the inhalations of oxygen by M. Paul Bert to overcome asphyxia, and used for the first time in that well-known and fatal ascension of the *Zenith* to a height of 28,216 ft., is an insufficient and incomplete means of maintaining life in these inhospitable regions. Closed and heated cars, in which the explorers shall be enclosed, have been proposed, but when we consider the expense of construction of a balloon equipped in this way, so as to exceed an altitude of 32,800 ft., it is not at all astonishing that it has not yet been put into execution. There nevertheless does exist a very simple means of reducing by enormous proportions the amount of aeronautical material by doing away with the aeronauts themselves, and of sending with simple little balloons some light instruments which, when they have returned to the earth and been found again, will show us what has occurred in the heights of the earth and ocean whither they have been sent.

This idea was promulgated by M. Claude Jobert in 1873, but has not yet been put into practice. Nevertheless, during the year 1892 M. Hermite made a series of experiments for the sake of accomplishing this object, in which he attempted to eliminate successively the difficulties which presented themselves. The first question to be solved was, Will we be able to find the balloons again which have been confided to the atmosphere? Will they not be carried by the upper currents at an enormous velocity and fall into desert lands or into the sea? Will we be able to recover the balloon and instrument intact that have fallen into the hands of people that are curious or ignorant? Experience alone can solve this problem. So while always busy in designing a very light registering apparatus, M. Besançon took it upon himself to construct small balloons of different cubic capacity and different substance, intended for these first attempts.

In the month of March, 1892, they started out almost every day several balloons provided with a circular of questions which contained their addresses, and which is reproduced below,

Ascension of balloon..............................
Started from......................................
The ....at ....hr.......min ....

NOTICE.

Whoever shall find this card is requested to send it to the mayor or the comptroller of the nearest commune and to fill out as far as possible the questions given below, and to send it without breaking it to the address on the opposite side.

Question No. ....Descended at........ ...hr. ....min. ....
1. At exactly what point did this card fall? .... Commune of ....Department of.................
2. At what time was it found? ....hr. .....min. ...
3. Has the balloon been seen? If it were seen, at what hour?
4. What was the temperature at the time the balloon passed?
5. What was the height of the barometer?
6. What was the weather and what was the appearance of the sky?
7. What was the strength and direction of the wind?

PERSONAL REMARKS.

....
....

Signature and address { .... / .... / ....

These balloons were sometimes provided with automatic distributors of cards in order to determine the velocity and the direction of their course. Several were returned, and they have given interesting results, to which we will refer later. It is sufficient to say here that the results were far better than was expected. About half of these little balloons, whose cubic capacity did not exceed a yard, were found again, and all fell at a distance from Paris comprised within a radius of 93 miles.

The second question to be solved was, What are the conditions to be fulfilled by a balloon in cubic capacity and construction which is suited to reach the maximum altitude? Theoretically, in order that a balloon may reach, for example, a height of 18,000 ft., where the barometric pressure is reduced by one-half, it is necessary that one-half inflated it shall be able to rise. To reach a height of 28,800 ft., where the pressure is reduced to one-third, it is necessary that it shall be able to rise when one-third inflated, and so on. Practically, however, this will not be the case, for if a balloon is given a very slight ascensional force the slightest additional weight will prevent its ascension. The causes for extra weight would be the escape of gas, cooling, and a deposition of dampness on the covering of the balloon.

By careful construction and choosing proper materials for the construction of the envelope, the factor of the escape of gas can be avoided; but the two other causes depend on

meteorological circumstances, and are independent of human control. To be sure, we can put the chances on our side by choosing a clear sky, but it is desirable that these explorations should be frequently renewed. It was necessary to determine, then, whether a balloon of predetermined capacity is on the average capable of crossing the snow zone, which is the true barrier against which the frail waifs of the air are compelled to hurl themselves if they are not furnished with sufficient force to cross it.

From experiments which were made on large balloons, the deposit of dampness which can occur on their envelope amounts to .25 oz. per square foot, a weight corresponding to a thickness of liquid water of .01 of an inch. Then, admitting that the diminution of the temperature of the gas of the balloon will amount to 54° F., the result will be a practical increase of weight of .02 oz. per cubic foot, for a balloon inflated with hydrogen, and .062 oz. per cubic foot for a balloon inflated with illuminating gas.

M. Hermite has taken as a typical envelope, triple goldbeaters' skin, which offers the advantages of strength, lightness, and almost absolute impermeability. Nevertheless, he has also obtained good results with a paper weighing .02 oz. per square foot. Wishing to keep within practical limits, and not enter the domain of fancy, he has not thought it advisable to make balloons of a greater cubic capacity than 147,000 cub. ft., for beyond 15.5 miles it is necessary to increase them to enormous proportions in order to gain a few miles. We can therefore fix on from 15.5 to 18.6 miles as the limit of aerial exploration by means of balloons. It now remained to put theory into practice, and the beginnings were not fortunate.

We give a table* showing the maximum changes of different balloons, and consequently the ascensional force given to them at starting, in order that they may attain their maximum altitude, a theoretical altitude, and a real altitude which is greater than a theoretical altitude, because the gas of the balloon heated by the sun has a temperature of about 54° F. greater than that of the surrounding air, and this difference in temperature ought to increase still more at higher altitude on account of the continual increase in solar radiation.

The first experiment of an ascension to a great height was made on August 7, 1892, at the gas works of Noisy-le-Sec, with a balloon having a capacity of 8,890 cub. ft., made of newspaper saturated with coal oil, and carrying a mercury barometer, which was automatically closed on the descent by an arrangement which was designed by M. Hermite. This apparatus with its load weighed 42 oz. It was desired to inflate this balloon without the net by simply introducing an amount of gas sufficient to balance the shell and the instrument, and giving it only a few pounds of ascensional force, the weather being fair. Unfortunately the flow of gas was far too slow, and we were obliged to give up the ascension. The balloon was rendered useless by handling.

* Page 370.

I.—TABLE OF EXPERIMENTS WITH EXPLORING BALLOONS.

DATE.	Kind of Balloon.	Volume.	Kind and Weight of Instrument.	Ascensional Force at Starting. Illuminating Gas.	Meteorological Observations on Starting.	Time of Departure.	Maximum Altitude.	Maximum Temperature.	Remarks.
1. Oct. 4.	Varnished paper, without net.	177 cu. ft.	Barometer and low registering thermometer, with automatic card distributor, 8.8 oz.	17.7 oz.	Cloudy; strong southwest wind.	11.20 A.M.	....	....	Balloon not found Started from gas-works of Noisy-le-Sec.
2. Oct. 11.	Gold-beater's skin without net.	13.2 cu. ft.	Barometer, 2.65 oz.	2.65 oz.	Cloudy; light southwest wind.	3.35 P.M.	3,987 ft.	....	Mont - Dauphin (S. and Marne), 47 miles from Paris.
3. Oct. 14.	Gold-beater's skin without net.	13.2 cu. ft.	Barometer, 3.9 oz.	.53 oz.	Cloudy; strong southerly wind.	1.40 P.M.	....	....	Plaine d'Ory (Oise), 24 miles north-northeast from Paris. Instrument injured by peasants.
4. Oct. 19.	Paper, .65 oz. per sq. ft., without net.	529.7 cu. ft.	Barometer, 4.2 oz., and automatic card distributor	Not determined. Balloons partially inflated.	Cloudy; rained after starting; northerly wind. Night ascension	5.50 P.M.	10,991 ft.	....	Chamarande (Seine-et Oise), 28 miles south of Paris.
5. Oct. 29.	Paper, without net.	177 cu. ft.	Barometer, 5.3 oz.	Almost nothing. Balloon partially inflated.	Cloudy; light wind from the south.	12.30	....	....	Fell in the Rue Paradis, 15 min. after the start.
6. Oct. 29.	Paper incompletely saturated, without net.	177 cu. ft.	Barometer, 5.3 oz.	17.7 oz.	Cloudy; light wind from the south.	3.55 P.M.	6,562 ft.	....	Fontaine (Oise), 26 miles from Paris.
7. Oct. 31.	Saturated paper, without net.	177 cu. ft.	Barometer, 5.3 oz.	7.05 oz.	Rain clouds, light southerly wind.	1.45 P.M.	....	....	Balloon not found again.

8.	Nov. 2.	Gold-beater's skin with net.	141 cu. ft.	Barometer, 4.2 oz.	Full inflation.	Clear; light southerly wind.	3.40 P.M.	28,500 ft.	....	Evry (Aube), 98 miles southeast of Paris.
9.	Nov. 14.	Gold-beater's skin with net.	141 cu. ft.	Barometer and low registering thermometer, with skin, 9.2 oz.	Full inflation.	Clear; light southerly wind. Barometer, 29.92 in.; temperature, 62.6° Fahr.	1.30 P.M.	24,935 ft.*	50° F.	Chanes (Oise), 37 miles from Paris.
10.	Nov. 17.	Gold-beater's skin with net.	141 cu. ft.	Barometer and low registering thermometer, with skin, 9.2 oz.	Full inflation.	Cloudy, with clear space. Barometer, 29.92 in., temperature, 57° Fahr, with a somewhat strong breeze from the south.	10.45 A.M.	26,908 ft.	64° F.	Goyencourt (Somme), 68 miles north-northeast of Paris.
11.	Nov. 20.	Paper, saturated with coal oil, with net.	177 cu. ft.	Barometer and low registering thermometer, with skin.	Two-thirds inflation.	Cloudy, with clear spots. Barometer, 29.94 in., 48° Fahr. Moderate wind from the southwest Night ascension	8.40 P.M.	21,654 ft.	66° F.	Goyencourt (Somme), 68 miles north-northeast of Paris.
12.	Nov. 25.	Paper saturated with coal oil and blackened, with net.	177 cu. ft.	Barometer, 4.06 oz.	One-half inflation.	Cloudy, with humidity.	3.25 P.M.	....	....	Rue de la Reunion, Paris. Barometer stolen by a gamin.
13.	Nov. 27.	Paper saturated with coal oil, with net.	177 cu. ft.	Barometer, 3.5 oz.	One-half inflation.	Cloudy; barometer, 29.94 in. Strong easterly wind.	3.10 P.M.	29,528 ft.	....	St. Florence in Vendee, 217 miles from Paris.
14.	Dec. 10.	Paper coated with Japan, with net.	2,119 cu. ft.	Barometer and low registering thermometer. Apparatus to register the power of the wind, 12.35 oz.	33 lbs.	Cloudy; light wind from the northwest.	1.40 P.M.	....	....	Balloon accidentally turned at a height of 984 ft. Fell near the Canal St. Martin. Instruments intact.

II.—THEORETICAL TABLE OF ASCENSIONS BY EXPLORING BALLOONS.

Diameter.	Volume.	Surfaces.	Weight of gold-beater's skin.	Weight of net.	Weight due to humidity.	Maximum weight on cooling.		Total charge.		Total ascensional force.		MAXIMUM ALTITUDES.							
												For balloons inflated with hydrogen.				For balloons inflated with illuminating gas.			
												Envelope alone.		With net and 10.8 oz. of instruments.		Envelope alone.		With net and 10.8 oz. of instruments.	
						Hydrogen.	Illuminating gas.	Hydrogen.	Illuminating gas.	Hydrogen.	Illuminating gas.	Temperature of the gas equal to that of the air.	With an increase of temperature of the gas of 86° Fahr.	Temperature of the gas equal to that of the air.	With an increase of temperature of the gas of 86° Fahr.	Temperature of the gas equal to that of the air.	With an increase of the temperature of the gas of 86° Fahr.	Temperature of the gas equal to that of the air.	With an increase of the temperature of the gas of 86° Fahr.
Ft.	Cu. ft.	Sq ft.	.24 oz. per sq. ft	Oz.	.82 oz. per sq. ft.	Lbs.	Lbs.	Lbs.	Lbs.	Lbs.	Lbs.	Ft.	Ft.	Ft.	Ft.	Ft.	Ft.	Ft.	Ft.
3.3	1847	33.4	.08	.28	.27	.024	.068	1.73	1.78	1.26	.86	24,016	24,442	1,526	2,034	13,910	15,846	......	......
6.5	148.23	134.5	.32	.42	1.10	.194	.56	7.078	7.44	10.2	6.9	41.667	42,455	34,449	34,908	31,690	33,892	24,279	26,378
9.8	519.2	304.5	.75	2.1	2 5	.68	1 94	16.3	17.5	35.6	24 3	53.478	54,102	49.213	49,869	43,307	45,440	39,170	41,960
13 1	1183.1	541 4	1.33	6 0	4.4	1.54	4.43	29.3	32.6	81.25	55.4	60,499	60.860	57,067	57,743	49,869	52,035	46.917	49.705
16.4	2306	845	2.07	14.1	6.1	3.24	8.64	46.5	51.9	158.3	108.04	66,241	66,602	63,321	63,780	55,775	58,071	52.822	54,985
19.7	3990 7	1216	2.98	28.2	9.9	5.28	14.9	67.5	77.2	274	186.85	70.867	71,852	67.586	68,570	60,860	62 993	57,744	59,794
32.8	18,469	3380	8 29	228.8	27.6	24.25	69	197 3	242.3	1268	864.8	84,647	85,434	77.757	78,577	74,803	76 609	68.018	69,980
65.6	147,900	13,520	33.16	3520	110 5	194	554	886.4	1246	1328	6926	104,663	105,318	83,990	84,647	94,161	96,458	73,655	75,461

The second attempt was made on September 8 at the Villette gas works. The balloon was smaller, and had a capacity of only 1,059 cub. ft., but the paper was lighter, as it weighed only .065 oz. per square foot; it was also stronger as well as more impermeable. In consequence of an accident, the balloon was torn just at the moment when it was properly filled, and it had to be thrown away.

They then wished to try another method of inflation by employing a net and conducting the operation as though they were inflating a regular balloon. This experiment was made on September 17 at the gas works of Noisy-le-Sec, with a balloon like the preceding one; the operation succeeded very well in spite of a violent wind, and the balloon, inflated to one-third of its capacity, carried the barometer at this time. Unfortunately, the weather was threatening, and soon after its departure the rain began to fall with so much violence that the balloon came down before it had reached the clouds. The instrument was found to be intact, and not a drop of mercury had escaped. These failures led to the opinion that, in order to facilitate the manipulations and diminish the weight of the barometer, it would be necessary to design a registering apparatus which would not weigh more than 3.5 oz.—that is to say, about one-twelfth as much as those which had heretofore been used; but, on experimenting, one has been made with a weight of only 1 oz. This instrument is composed of a vacuum box carrying a vertical sheet covered with lamp-black. By the gradual inflation of the box a vertical line proportional to the altitude attained is drawn by a steel stylus resting against the glass plate. In order to determine the altitude with precision, the apparatus is put under a pneumatic pump and a vacuum produced until the stylus reaches the top of its mark again. By measuring the vacuum produced with a mercury manometer, the maximum altitude reached by the balloon is determined. The measurements thus obtained are very exact. In fact, on October 19, 1892, in the long ascension of the balloon *Le Journal*, one of the apparatus was given to M. Besançon, who supervised the ascension, and he returned it in a sealed box. M. Hermite then proceeded to the verification of his apparatus by the method described above, and found the maximum altitude of 10,663 ft. indicated by the registration furnished by M. Jaubert, Director of the Observatory of the Tower of Saint-Jacques.

The first experiment with this system took place at the gas works of Noisy-le-Sec on October 4, 1892. The light paper balloon had a capacity of only 177 cub. ft. To the small instrument a very light, high and low registering thermometer was also added. The whole was put in a box (fig. 2) and protected from solar radiation by a skylight. It carried also four circulars of inquiry, which were set free successively by the automatic burning of a holder. This time the balloon rose rapidly and went toward the northeast, and no news was ever heard of it or the instruments.

M. Hermite then began again, and this time succeeded. On

October 11, 1892, a small balloon of gold-beaters' skin was inflated with illuminating gas. It had a diameter of only 3 ft., and carried a registering apparatus weighing 5.3 oz.

Two days later he received by post the balloon and the instruments, which had been picked up in the commune of Mont-Dauphin, 98 miles east of Paris. He immediately put the barometer under the pneumatic pump, and found that the maximum altitude attained was 3,987 ft. Since that trial there has been an almost uninterrupted series of successful experiments, soon reaching a height of 9,842 ft., then 26,247 ft., and then finally an altitude of 29,528 ft. with balloons of 141 and 176 cub. ft. capacity only.

Fig. 1 gives a map showing the approximate course of these little balloons. We see a tendency to move toward the east, as has also been observed on those rare ascensions which aeronauts have made to a great height. As for the rapidity of the currents, it does not appear to be very great. A single ascen-

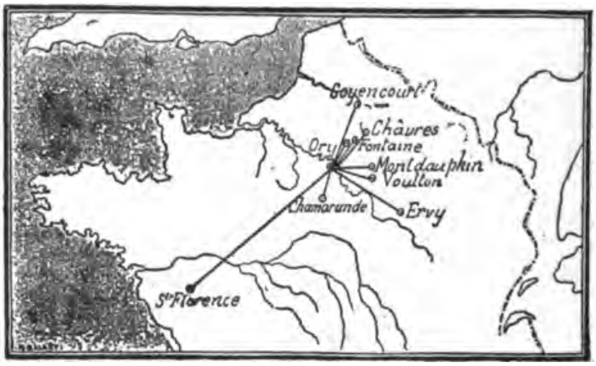


FIG. 1.—MAP SHOWING ROUTES OF EXPLORING BALLOONS.

sion is an exception to the rule both in regard to speed and direction—that of November 27, 1892, when the balloon, after having reached an altitude of 29,528 ft., was picked up 100 leagues west of Paris, in Vendee, not far from the sea-shore.

Fig. 2 gives a half-size illustration of the instruments sent out on the ascension of November 14, 17, and 20, 1892, and which were composed of a vacuum-box, *B*, whose inflation is multiplied by the lever carrying the stylus *S*, which marks, during the ascension, upon the vertical sheet of glass *P*, a line proportional to the altitude attained. At the bottom of this instrument there is a small thermometer marking the maximum and minimum temperatures. This apparatus is fastened upon a strip of wood attached to the box. The box is covered by a sheet of glass in order to protect the instruments from the investigations of the curious. A note on the

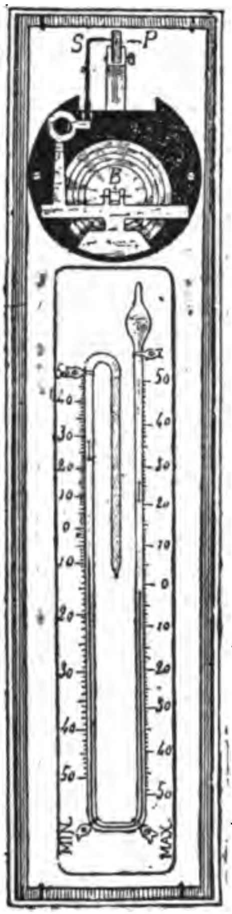


FIG. 2.

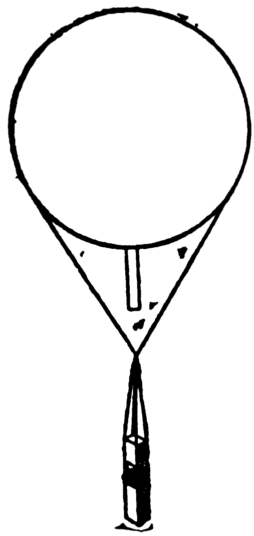


FIG. 3.

cover facilitates the reading of the thermometer, which should be done, if possible, at the time when it descends.

In all these three experiments the instruments were returned in perfect condition.

Fig. 3 shows the little balloon fully inflated and carrying the box into the upper atmosphere. As to the temperatures thus far noted they do not represent anything very new—80°

F. below zero for 24,935 ft., 90° F. below zero for 26,903 ft., and 82° F. below zero for 21,654 ft. in height, or an average of 510 ft., 474 ft., and 430 ft. for each degree F. It may be remarked, furthermore, regarding the ascension of November 17, 1892, that the gold-beaters' skin balloon, having a diameter of 6.56 ft., whose total weight is given in Table I., reached an altitude of 26,903 ft. instead of 26,378 ft. This seems to be confirmed by remembering that the difference between the temperature of the air and that of the gas increases with the altitude.

Such is the *résumé* of these ascensions, which are only rough trials.

As several people have lately announced their intention of sending up balloons, M. Hermite published his first experiment in order to establish a priority, not as an invention, but in application of a method which may be called upon to render great service to the science of meteorology.

Hereafter we shall give an account of the experiments which were made with a balloon of 399 cub. ft., carrying thermometers and barometers with a registering clock movement to a height of 59,056 ft., and only weighing 10.6 oz., as well as an apparatus for taking samples of the air, and another intended to collect the dust in the air.

The publication of the foregoing in the *Comptes Rendus* of the Academy of Sciences brought about the publication of an article by Commandant Renard, of Meudon, also in the *Comptes Rendus*; the author depends upon certain long mathematical calculations to prove that these test balloons could not pass certain altitudes. I do not think that it is necessary to discuss these criticisms, for the following reasons: La Place's formula is of no practical value except within the limits of the experiments which were made by Baron in the Pyrenees, which were published in the "Memoirs on the Fundamental Formula of Celestial Mechanics and the Conditions of the Atmosphere which Modify its Properties," which were made at Clermont-Ferrand in 1811. Their experiments were only carried to a height of about 9,843 ft. The members of the Bureau of Longitudes, which inserted it every year in their annual, have taken great care not to apply it to heights greater than 26,247 ft., and they accompany the publication of their table with all kinds of reservations. It is, therefore, absurd to use it as an authority for heights two or three times as great. Starting from these altitudes, which are so different from the previous observations, the formula is of practically no value. It is, therefore, necessary to renounce the possibility of making an application of it from thermometric measurements. If this were done it would only require a slight exertion of the imagination in order to obtain a misleading result, and one which would merely deceive those who are ignorant.

Furthermore, the Academy of Science shared in this opinion in regard to the slight value of La Place's formula (a proof of which will be found in the report made on January 15, 1872, by Messrs. Becquerel, father and son, Dupuy de Lome,

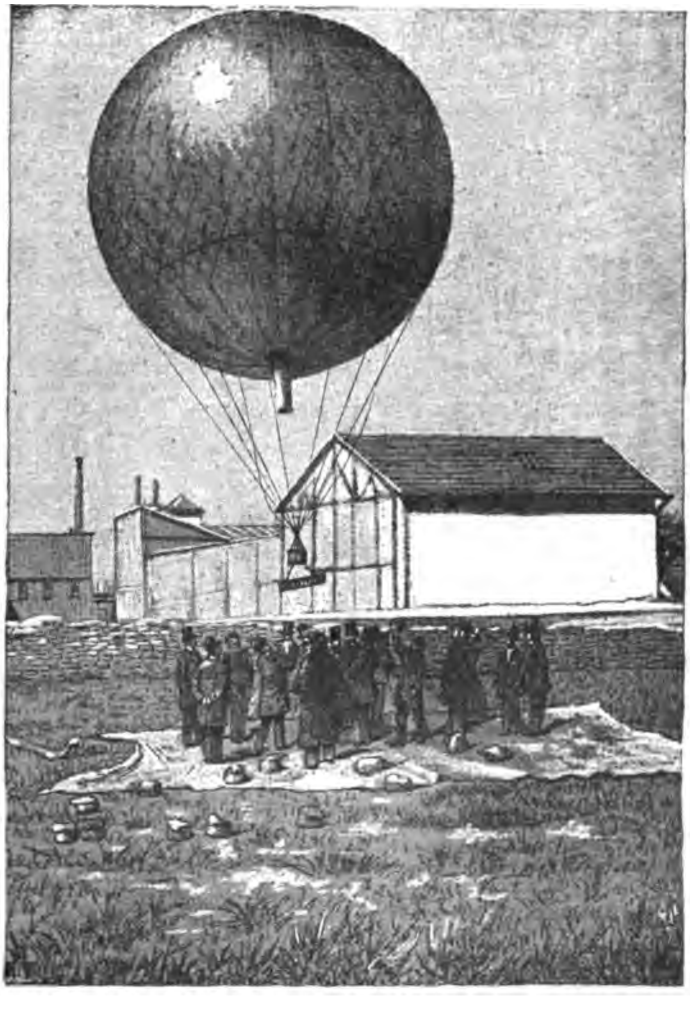


FIG. 4.—ASCENT OF EXPLORING BALLOON FOR RESEARCHES IN THE UPPER ATMOSPHERE.

Renault & Leverrier) upon these aeronautic experiments which were then projected (see *Comptes Rendus*).

The ascent of these testing balloons should always be accompanied by trigonometric measurements taken from one or two stations on the surface of the earth. This is the opinion of Mr. Janssen, who has made the necessary arrangements to do this, and who has placed himself at the service of experimenters.

Furthermore, that the heights given in the table have been very greatly exceeded in the experiments, there can be no question when calculated by the formulæ which we have just attacked, but which can be used without any harm provided a merely conventional value is attributed to them, which will be further removed from the truth as the altitude increases.

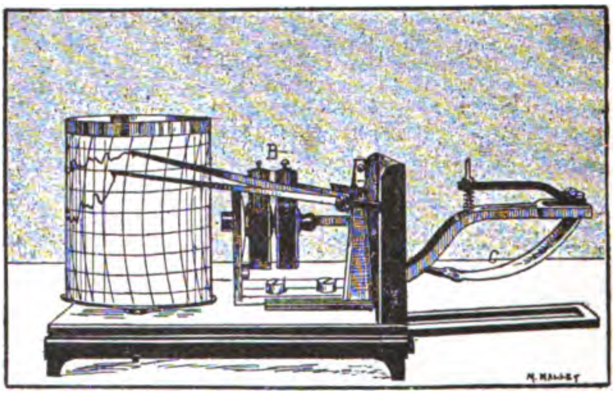


FIG. 5.—REGISTERING APPARATUS.

The story of the last ascension is given in the following notes written by Mr. G. Hermite :

In a previous number of *L'Aerophile* I announced that I would send up a gold-beaters' skin balloon having a capacity of 4,000 cub. ft., carrying with it beyond actually known limits a registering apparatus for taking samples of the air, and another intended to gather the air dust. This experiment, which was delayed for a long time in consequence of the bad condition of the atmosphere, and also by the construction and graduation of the registering apparatus, took place on March 21st, 1893, and was favored by magnificent weather. Before giving the details of this ascension there are a few indispensable matters regarding the balloon and its apparatus regarding which it is desirable to speak. The balloon *L'Aerophile* is spherical and is 19.7 ft. in diameter ; it is constructed of gold-

beaters' skin of triple thickness, varnished inside and out; it is almost absolutely impermeable, and its strength permits it to resist all the manipulations which are necessary in inflating it. At the top pole there is a small circle of about 5.9 in. in diameter which serves as a point for attaching the net and for fastening the apparatus. At the bottom pole there is an opening 11.8 in. in diameter provided with a handle 3 ft. long. The total weight of the balloon is 24.25 lbs. Its exterior surface is 1,216 sq. ft. The weight of the envelope per square foot is a little less than .328 oz. The net is made of Brittany linen; it weighs complete with suspension cords 2.2 lbs., and its strength is 3,528 lbs. The instruments, fig. 5, which were attached to this balloon were:

First, a barometer and thermometer registering upon one and the same cylinder. This arrangement permits of the lowering of the weight and volume of the registering instruments by one-half. The barometer is connected with two vacuum boxes, *B*, which are set for a height of 72,200 ft., and the thermometer, *C*, which is made on the Richards system, is graduated to note 90° F. below zero. The height of the cylinder, which turns once every four hours, is 2.8 in. This apparatus, which might be called a barometric thermograph, weighs 3.52 lbs. It is mounted in a closed metallic box. In order to protect it from shocks the box is fastened on rubber buffers in a small wicker basket weighing 1.1 lbs., the case of which is closed by four clasps. The total weight of the apparatus is 4.62 lbs. The second apparatus consists of another barometric thermograph which is smaller than the first, the principal parts of which are made of aluminum and which only weigh 10.58 oz.; the height of this cylinder is 1.6 in., and its period of rotation is 5 hrs. 22 min. It is encased in a small wooden box provided with rubber cushions; the total weight is 17.69 oz. The inscription on the paper of the cylinder was made with a special ink manufactured by the Richard Brothers. Both apparatus contained the following notice:

"AEROPHILE UNION OF FRANCE.

"*Explorations of the Upper Regions of the Atmosphere.*—Any one who shall find this balloon and this box is earnestly entreated to take it at once to the mayor or the comptroller of the nearest commune, who will send immediately to the following address a telegraph despatch which will be paid for, as follows:

"Aerophile Union of France, 113 Boulevard Sebastopol, Paris.
"Balloon found at................................
"Signature and address.... ....

"A member of the Aerophile Union of France will immediately start, upon the receipt of this despatch, to take possession of the instrument and to recompense the person who has found the balloon. We beg, furthermore, on account of the

scientific importance of this experiment, that the finder will not attempt to open the box or basket, which contains meteorological instruments which are very fragile, especially as the opening of this box will render the experiment, which has been very costly and troublesome, of no avail. A liberal reward will be given to those persons who follow out this instruction."

The other instrument was a distributor of the question cards, consisting of a horizontal box to which an automatic distributor was attached through which a thread was drawn and upon which the packages of cards were suspended.

By the burning of the fuse the packages are successively set free and fall, flying off into space. They should be sent back to their destination. There are about 600 of these cards, weighing altogether 7.7 lbs.

Finally, in order not to overdo the matter in the first test, the apparatus for taking samples of the air and gathering the dust was done away with. The object of this ascension was to determine the temperature of the air beyond known limits, and to compare it with that of the gas by means of a small barometric thermograph suspended in the interior of the balloon in order to measure the intensity of the heat radiation.

The whole of this material, whose total weight was 37.5 lbs., was taken to Vaugirard, where, in the aerostatical park of M. Lachambre, there was a hydrogen generating apparatus that was used to inflate the interesting dirigible balloon of M. Le Compagnon. We would have liked very much to have filled *L'Aerophile* with hydrogen in order to have it ascend to its maximum altitude. Unfortunately, owing to the insufficiency of the apparatus and its output, after several hours of chemical manipulation this idea had to be given up. At 11.30 we decided to use illuminating gas.

The experiments of the past year had taught us the best method of starting, which consists in letting the balloon go when it is entirely full of gas with the whole of its ascensional force. We thus do away with the complication of an automatic unloader, and the maximum altitude is obtained with great rapidity. It is necessary to take the precaution to fill the balloon and to make a careful examination of the envelope to see that it is quite solid and will not tear, as happened to us on December 10th, 1892, with a Chinese paper balloon of 2,295 cub. ft. capacity, which was not entirely filled. This disaster is easily explained by the resistance of the air, which suppresses the upper portion of the balloon and drives the gas down toward the opening, and stops the ascensional movement; then the gas remounts toward the upper portion, to be again pushed back anew when the balloon has reached some velocity; there are, therefore, deformations produced in the form of a balloon, and shocks, which cause the tearing.

If the balloon is full there is no need of fearing this accident, since the interior pressure of the gas will hold the ex-

RÉSUMÉ OF OBSERVATIONS OF THE ASCENSION OF "L'AEROPHILE."

Time.	Indicated Pressure on the Diagram (Inches of Mercury).	Corresponding Height not Corrected for Air Temperature in Feet.	Temperature, Degrees Fahr.	Vertical Velocity, Feet per Second.	Decrease in Temperature with Height, 1° Fahr. per	Remarks.
12.25	29.7	213	63	......	......	Start from Paris (Vaugirard).
12.30	21.6	8,530	39	28.2	350	
.35	15.8	16,730	14	27.2	337	
.40	12.0	23,930	—11	24.6	321	
.45	8.5	33,000	—40	30.2	318	
.50	6.3	40,910	—60	26.4	....	Intense solar radiation causes thermograph to read too high.
.55	5.2	45,930	—55	16.7	....	
.57	......	....	—52	....	....	Interruption of the diagrams by the freezing of the ink at —67°.
14.30	4.06	52,500	[—104]	....	[318]	Beginning of the barograph trace.
15.15	4.1	52.280	....	....	...	
16.05	.....	50,850	— 6	....	....	Beginning of the thermograph trace.
17 11	4.7	48,560	—10	....	....	
17.21	......	....	—20	....	....	Barograph trace stopped.
17.31	......	....	—21	....	....	
17.41	......	....	—28	....	....	
17.51	7.1	....	—36	....	....	
17.55	......	[37,730]	—41	....	....	Approximate height.
18.01	.....	....	—44	....	....	
18.11	9.1	31,170	—53	....	299	Sunset at Paris, but not at balloon.
18.21	......	....	—40	....	....	
18.31	......	....	—15	....	....	
18.41	......	....	7	....	....	
18.43	18.8	12,140	9	....	....	Barograph trace begins.
18.45	19.5	11,160	14	8.2	301	
18.50	21.8	8,860	19	7.5	275	
18.55	23.1	6,730	27	7.1	278	
19.00	24.8	4,920	31	6.0	242	
19.05	26.7	3,020	38	6.2	224	
19.11	29.58	328	50	7.5	...	Landing at Chanvres, 77.7 miles s. e. of Paris.

terior pressure of the air in equilibrium. It is only necessary to take the precaution of giving the gas a sufficient orifice for exit. The inflation of *L'Aerophile* was accomplished just as that of any lifting balloon.

The haste with which this operation was accomplished in order to make up for the time which was lost was injurious to the success of the experiment. A false movement and some gusts of wind caused a rupture of several meshes of the net,

which was, nevertheless, very strong. Fearing an approach of a more violent gust, we rapidly fastened the two barometric thermographs to the suspension cords, but forgot the sun screen. At the bottom of these instruments a distributor of the cards was suspended lighted at both ends. At 12.20 the balloon was entirely filled with gas and started with all its ascensional force, which was about 143 lbs. *L'Aerophile* rose with great rapidity, and started first toward the northwest; then, gradually changing its direction by a kind of spiral movement of the atmosphere, it took a westerly direction, and finally, at a height which we estimated to be 32,800 ft., it appeared to come back with great speed toward the east. It then appeared like a brilliant star and seemed to approach the zenith, which proved the rapidity of its ascensional movement. It remained visible to the naked eye for about three-quarters of an hour, and then clouds appeared in the lower regions to obstruct.

The next morning we received this telegram :

To the Aerophile Union of France, 113 *Boulevard Sebastopol, Paris :*

Balloon found at Chanveres, Joigny (Yonne).

TRUCHON, *Comptroller*.

I immediately went to the place indicated, and there I learned that the balloon had been seen about a quarter of an hour before it reached the earth by a large number of inhabitants of Joigny and Chanveres. It was entirely full and descended very slowly. It came from the northeast ; the inhabitants thought that this was a regular ascensional balloon coming from Troyes.

It was caught by a young man in a rye field, after having made three jumps of 164 ft. each. The time from its departure was about 7½ hours. Soon afterward a crowd of boys gathered about it and tore the balloon in order to cause it to collapse more quickly. Fortunately the mayor and comptroller, who were near by, arrived during this performance, and had the material taken into one of the school-houses, where I found the balloon and instruments. I proceeded at once to open the basket containing the main register. The instruments were in perfect condition, and had not suffered anything from the abuse of the balloon. The diagram was somewhat complicated in consequence of the duration of the voyage of the balloon, which had caused some of the lines to be traced over those first drawn. It was for this reason that the first telegram which I sent to Besançon, who remained in Paris, indicated the height as 50,850 ft., a little less than the actual barometric altitude as determined by a later and more careful inspection of the diagram, and the minimum temperature was 59.8° F. below zero. As for the small model apparatus, it had simply marked the vertical lines which indicated the maximum and minimum points. The card distributor had burned out ; the fuse had burned from the two ends over a length of 9.4 in.; and three cards still remained attached. On

my return I proceeded immediately to the verification with a pneumatic pump of the barometric diagram, and I saw that the barometer had acted very well indeed. The reconstruction of the diagram has been a very long and delicate piece of work, and we reproduce it exact in fig. 5. According to the first traced lines of the barometric diagram, we see that the balloon ascended with tremendous velocity. It started at 12.25 in the afternoon, and at 12.30 it had reached a height of 8,530 ft., which gave it an ascensional velocity of 28.2 ft. per second. This speed of ascension corresponds very well to the general formula which is admitted for resistance of the air upon spherical balloons. In fact, the pressure of a wind of 26.25 ft. per second upon a plain surface is .514 lbs. per square foot, which, multiplied by 904.2 sq. ft. of the great circle of the balloon, gives us a total of 464.75 lbs., which is equal to nearly four times the ascensional force, which is 132 lbs. Thus for a spherical balloon of 19.7 ft. in diameter, the resistance of the air is equal to one-quarter of the surface of the great circle.

At 12.35 it had reached a height of 16,730 ft.; the ascensional force slowly decreased ; it was then 27.2 ft. per second ; at 12.40 the aerostat had reached an altitude of 23,951 ft., and its vertical velocity had dropped to 24.6 ft. per second ; at 12.45 it was at 33,016 ft., and its speed had increased to 39.2 ft. per second. This increase of speed must be attributed to solar radiation. Then, as the balloon ascended, its ascensional force diminished with the same proportion as the density, and, consequently, the resistance of the air. There is, therefore, this compensation, and the speed should be constant and uniform. Nevertheless, about its maximum altitude the ascensional force diminished, and ended by disappearing entirely ; the resistance of the air checked it, and the speed decreased rapidly. Then there was an increase of speed over a portion of the trajectory, and there was an increase of ascensional force due to the heating of the gas under the influence of solar radiation.

At 12.50 the balloon had reached a height of 41,011 ft., and the speed had lowered to 26,247 ft. per second. At 12.55, 30 minutes after its departure, the aerostat showed a barometric depression of 5.12 in., corresponding to an altitude of 45,932 ft., and its ascensional speed was reduced to 16.7 ft. per second. Here the diagram is interrupted, and this accident should be attributed to the congelation of the ink, which took place at 67° F. below zero, for the registering pens were filled full of ink and pressed closely upon the paper ; and we will see by an examination of the thermometric diagram that this explanation is confirmed.

The diagram was resumed again at 2.30, when the mercury pressure was 4.05 in., or a height of 52,494 ft. had been attained under the influence of solar radiation, which had not only penetrated the paper, but heated the metallic box of the barometric diagram. Under this intense radiation the balloon maintained itself for two hours, floating in this high atmosphere without changing its altitude, in which there was noth-

ing astonishing when viewed from the fact that there was a complete absence of vapor in the atmosphere, and that the sun was still high above the horizon.

About 3.15 *L'Aerophile* manifested a tendency to descend; at 4.25 the barometric diagram still marked 4.3 in., indicating a height of 50,526 ft. At 5.11, at an altitude of 48,557 ft., there was a second interruption of the diagram, corresponding to a lowering of the temperature, evidently due to a diminution in solar radiation. Nevertheless, in descending the balloon passed through warmer currents of air, and the barometric diagram was resumed at an altitude of 12,139 ft., indicating an average speed of descent at 7.87 ft. per second. The landing took place at 7.11 in the evening, the voyage having lasted 6 hours, 46 minutes.

We will now pass to the inspection of the diagram formed by the thermometer. At the starting the thermometer indicated 58° F. We see, at first, that the temperature dropped regularly. At 8,530 ft. it was 39° F., which was a diminution of temperature of about 1° per 610 ft. At 16,733 ft. the thermometer indicated 14° F., being a more rapid decrease, or 1° for 328 ft. Then it is still more rapid at 23,951 ft., where it reached 11.2° F. below zero, or 1° for each 288 ft.

At 33,000 ft. the thermometer indicated 40° F. below zero. Here the decrease was only 1° for 359.61 ft. We see that the diminution in the decrease of the temperature coincides with the increase of the speed of the balloon. It thus shows that the solar radiation began to heat the instruments, and the actual temperature must have been somewhat lower. At 40,915 ft. we obtained the lowest temperature marked upon the diagram; that is, −59.8° F., or a lowering of 1° per 397.35 ft.; beyond this altitude the temperature rose. This anomaly is explained by the effect of the solar radiation, which is always becoming more intense. At 12.57 it is only −51.7° F., and the balloon should have been at a height of 47,572 ft. Here the diagram stopped by the freezing of the ink. Why did the ink not congeal at the lower temperature of −59.8° F.? The explanation is this: the registering pens were in a metallic box, and they were in the shade, the solar radiation not yet having had time to heat the box. The ink of the pens was, therefore, at the temperature of the surrounding space, while the tube of the thermometer was influenced by a radiation through the basket-work. The thermometric diagram was resumed at 4.05 o'clock. The balloon was then at a height of 49,218 ft., and the thermometer indicated −6° F. The influence of the solar radiation is more manifest here. It had not only heated the basket-work and the unrenewed air which it contained, but also the box of the thermometer where the ink was then thawed. It may be asked why the pen of the barometer began its work later than that of the thermometer. The reason is a very simple one. The pen of the barometer being strongly pressed against the registering cylinder, it would undoubtedly have made a mark if the ink had not been congealed; but the pen of the thermometer did not touch the

paper at all. I thought proper to take this precaution in order that no false thermometric indications might be made, since the mechanism of the thermometer was not as strong as that of the barometer. After the freezing of the ink of the thermometer the pen was dried up. To start it afresh a shock was necessary, and this shock occurred at exactly 4.05, at the time when the pen had to pass over a little brass bar which served to hold the paper upon the registering cylinders. An apology is necessary for entering into these minute explanations, but they are necessary for a complete comprehension of the diagrams.

For an hour the temperature indicated on the diagram oscillated between 4° and 5.8° F. Then at 5.05 the thermometer descended progressively until it again reached the lowest temperature of 52.6° F. below zero at the moment of sunset, which was at 6.11 in the evening. This temperature corresponds to a probable altitude of 31,169 ft., or 1° for 302.57 ft. Starting from 12,139 ft., where the barometer diagram is resumed, we have the following temperature: at 12,139 ft., 9° F., or 1° per 291 63 ft.; at 11,155 ft., 14° F., or 1° for 309.86 ft.; at 8,858 ft., 26.6° F., or 1° for each 289.81 ft.; 6,726 ft., 31° F., or 1° per 286.16 ft.

The decrease in temperature was greater on the descent than on the rise, since the temperature of the air diminishes with the height of the sun above the horizon.

Finally, after the ascent and descent, the indications given by the registering thermometer should be considered to be very nearly exact, since the two acted as a thermometric diagram by the ventilation of air through the basket-work. But during the period of stagnation in the high atmosphere, the air in the basket-work was not renewed. We should then conclude that its temperature should be very nearly that of the aerostat, which was also under the influence of solar radiation. This fact is of capital importance, for it permits us to learn what the actual temperature of the air is at an altitude of 52,494 ft. Then, if we undertake to determine what the difference between the temperature of the gas of the aerostat and that of the surrounding atmosphere was at the moment of maximum altitude, we will have the temperature of the surrounding air, and then we will already know that the actual temperature of the gas was 4° F. below zero. Now, nothing is easier than to determine this difference in temperature.

For example, let P be the total weight of the material and F the total ascensional force of the aerostat; Q, the pressure at which the aerostat is in equilibrium, admitting that the temperature of the gas is equal to that of the air, which is $\frac{F}{P}$.

Now $P = 34.76$ lbs., and F, admitting that illuminating gas has an ascensional force of .05 lb. per cubic foot, is equal to 164.3 lbs.; then $\frac{164.3}{34.76} = 5.363$. The aerostat would therefore

have been in equilibrium at an altitude when a barometric pressure was reached equal to $\frac{1}{5.363}$ of the atmosphere, and at a height of 5.7 in. mercury, which corresponds to an altitude of 43,291 ft. But the aerostat came to an equilibrium at a height of 52,494 ft., or 8,202 ft. higher at an altitude where the barometric pressure was 4.05 in. of mercury, or $\frac{1}{7.38}$ of the atmosphere.

If we designate this number by Q^1 and let F^1 be the ascensional force of this heated gas, we will have $F^1 = P \times Q^1$. Now, the value of P is not changed. We then have $34.76 \times 7.38 = 256.53$ lbs. It now remains to determine what temperature is necessary to produce this ascensional force; it is 252° F.

The coefficient of the expansion of gas being .002034, an expansion of $252 \times .002034 = .51257$.

The weight of a cubic foot of illuminating gas is .0343 lb.

The diminution of weight of 1 cub. ft. of illuminating gas will be $.034 \times .51257 = .017$ lb., and its weight will be $.034 - .017 = .017$ lb. The ascensional force will therefore be .0583 lb. per cubic foot. Multiplying by 3,950, the total volume of the gas, we have 230 lbs., which is within a very few pounds equal to the value of F^1. Thus the temperature of the aerostat was 252° above that of the surrounding air; and as this temperature was 40°, or very nearly below zero, it follows that the temperature of the air at 52,490 ft. is −256° F., which makes a difference of temperature of 319° between that of the earth, or a decrease of 1° F. for each 165 ft. This decrease in temperature would be still more rapid as the altitude increases. If we use this coefficient of decrease as a base, we will find that the temperature of absolute zero, or —459.4° F., will be reached at a height of 85,808 ft.—that is to say, at a barometric pressure of .78 in. of mercury. This altitude may perhaps be reached by these exploring balloons.

We reach the same conclusion in using the formula admitted for the barometric corrections due to the temperature of the air: $a \frac{2(T + T^1)}{1000}$. If we perform this calculation by admitting *a priori* a difference of 288° F. for a pressure of 4.05 in. of mercury, we find that the actual density of the air would be at this pressure equal to that of the air at 44,292 ft. of altitude taken at 32° F. or 0° C.

As to the true altitudes, they can only be actually determined with precision by astronomical observations. An interesting fact to be stated is the intensity of the radiation in this rarefied air or atmosphere. If we admit that at the earth the temperature of the aerostat under the direct rays of the sun should be 36° above that of the air in which it is immersed, an experiment made in an ascension to a great height of the

zenith, this difference of temperature is seven times greater at 52,500 ft. when the heat radiations are seven times as intense. The density of the atmosphere being reduced at this altitude to one-seventh, the radiation will be proportionally to the density of the air, but that is perhaps merely a simple coincidence of figures. Is this diathermanosity of the atmosphere not due to the almost complete absence of the vapor of water? This hypothesis, if it were verified, would explain the results obtained by Tyndal in his experiments on the diathermanosity of gases.

The experiment of March 21 can also furnish us with some data regarding lunar meteorology. In fact, *L'Aerophile* rose into a zone where the density of the air is less than that which remains in the great planes of the moon. If we admit that the density of the surface of a planet or a satellite is equal to the proportion of the weight at its surface, we know that a body will weigh only one-sixth as much on the surface of the moon as it does on that of the earth; hence the density of its atmosphere ought to be six times less than at the level of the sea on the earth. There is no astronomical observation which will contradict this hypothesis. It should be remarked, however, that the mercury, if it were taken to the surface of the moon, would stand at the same height as it does upon the earth, since the weight of the mercury is reduced in the same proportions as that of the air; but an aneroid barometer which is not subjected to the loss of weight would indicate the actual pressure of the atmosphere. The pointer of this aneroid would indicate a pressure of 5.1 in. of mercury, which corresponds to an altitude of 45,982 ft. from the surface of the earth. The balloon exceeded this altitude by 1.2 miles. The temperature on the surface of the lunar plane need, therefore, be that of the terrestrial atmosphere at a height of 45,982. Now, this temperature is about —202° F. As for the soil on the moon subjected to a radiation six times as intense as that upon the earth, it should have a temperature of about zero on our scale —Centigrade scale. While we might admit the possibility of the existence of certain plants, when it comes to the question of other animate beings besides plants, it would be necessary to inquire how they are constructed if they do exist in order to conform to climatic conditions, of which there is nothing analogous on our earth, especially when we consider their long night of 354 hours, which would bring the nocturnal temperature by radiation down to about —390° F.

It is, nevertheless, well to remark that the altitudes furnished by the barometer in this test do not tally with the corrections, $\frac{2(T + T')}{1000}$, admitted for the temperature of the air. This correction should not be neglected. Thus, if it is calculated for an altitude of 41,802 ft., where the indicated temperature was —59.8° F., we would find a diminution of 2,890 ft. in height. I think that the correction of the cold ought to be introduced into these calculations, although the absolute cer-

tainty of it is not fixed. An examination of the different conditions of the cold of the upper regions of the atmosphere has led me to certain considerations which I think will be interesting and which I will attempt to unfold, and show what led me to the opinions which still remain to be developed.

TEMPERATURE OF THE PLANETARY MEDIUM.

The intense cold of the temperature of the air has always excited surprise, especially among men living in the torrid zone.

How would it be possible not to be struck by the contrast which the snow slopes of the Himalaya Mountains present to the burning plain watered by the Ganges? The spectacle of the Alps and Pyrenees provoked the astonishment of our ancestors in a lesser degree even before the arrival of the Roman. Physicists believed that the upper atmosphere was an eternal vacuum until the invention of balloons permitted them to make an actual examination of it. They even exaggerated the dangers of the cold and the rarefaction of the air, and took the precaution of having the first aeronautical ascension made by animals before feeling justified in sending up men. Pilatus and the Marquis of Arlandes sent up a cock, a duck, and a sheep. It was ascertained after the attempt made in 1887 by Messrs. Jovis and Mallet with the *Horla*, that the zone within which life can be sustained does not really extend above 23,000 ft., and that it is impossible to go to greater heights without immediate danger of suffocation and death. But man knows no limit for his investigations, hence there has always been a desire to investigate the temperature of the upper regions of the air and even the space in which the earth describes its orbit about the sun.

The experiments, therefore, of Messrs. Hermite and Besançon are destined to find a solution of one of the problems which has attracted the attention of scientific men, yielding a knowledge more profound and more important than any it now possesses.

The theory of the mechanical equivalent of heat led Hirn and certain others to admit that an absolute zero of temperature does exist—that is to say, a thermometric degree below which objects are entirely deprived of heat; the internal movement, according to these physicists, which constitutes heat being completely paralyzed. This conception does not appear to be very logical. In fact, since there seems to be no limit to the increase above zero of the temperature of the thermometer, it is not very clear why there should be any limit to the lowering of the same. The considerations upon which the authorities who advance this idea rest are not such as to diminish the instinctive doubt which the mind has for accepting any such limitation. It is true that the coefficient of the expansion of all gases is $\frac{1}{273}$ of their volume under normal pressure of 30 in. of mercury—that is to say, that if the temperature of any gas whatsoever rises from 0° to 1° C., its volume will be

increased by $\frac{1}{273}$ of its original volume, and will become $V' = \frac{V}{273} + V$. But there is nothing to prove that by this operation we would have increased by $\frac{1}{273}$ the quantity of heat contained in the gas. There is nothing to prove, furthermore, that we have diminished by $\frac{1}{273}$ this quantity of heat if we cause the temperature of the same gas to descend from zero to 1° below C.

There is nothing to prove, furthermore, that in starting from t° below zero, and dropping to (t° + 1) whatever may be the value of t, that we always subtract $\frac{1}{273}$ of this known quantity, so that it would be completely exhausted by 273 similar operations. On the other hand, everything seems to show that it is nothing of the sort, and that the quantity of heat varies in other ways than in proportion to the increase of volume.

In fact, there is probably no gas which would not reduce to a liquid state before reaching 273° below zero C. The works of M Caillet seem to prove that this abstract theory cannot be applied either to oxygen or nitrogen—that is to say, to the constituent elements of air. The idea that the temperature of the planetary medium would be precisely equal to this thermometric degree, because there does not exist in the upper regions of the atmosphere any matter which contained heat, rests upon no certainly ascertained fact, even admitting that the existence of a planetary vacuum has been demonstrated.

At the beginning of this century, Joseph Fourier, formerly Secretary of the Egyptian Institute, presented to the first class of the National Institute a series of papers on the mathematical theories of heat which arrived at different conclusions, and which certainly merit our attention as well as these. This investigator, who was afterward a prominent secretary to the Academy, started out with the idea that the earth is an old burned-out sun, which contains in its interior, at a depth of a few miles, a true furnace, in which the most refractory substances are transformed into incandescent lava raised to an unknown temperature. From this he developed equations which permitted him to determine the energy of the flow of heat which each year passed through the solidified strata upon which the basin of the seas rested. These equations rested only on the knowledge of the increase of temperature which has been observed down to a certain depth. They permitted him to show, in developing his theory, that the value of the flow is very far from the average which was attributed to it in the preceding century. They showed that the heat, in coming from this central furnace, only increased the average temperature of the earth by $\frac{1}{30}$° C.—that is to say, by an amount so small that it escapes all notice of investigators.

The results of this theory are easily understood. All the heat which we possess is derived from the sun. The old burned-out nebula upon which we live gives us practically nothing at all. It is the sun which heats the earth just as it

lights it. But this ingenious physicist did not limit himself to theoretical speculations. He tried to take a step further. He called attention to the fact that the poles of the earth were turned away from the sun for six months, and, consequently, that these points were exposed to nocturnal radiation for a very long time, from which he was led to believe that at these inaccessible points the temperature of the planetary medium would be reached. The determination of a coefficient so important to celestial physics will probably be the result of the explorations which Lieutenant Peary, Mr. Johnson, and Dr. Nansen will accomplish in the course of the coming year.

Joseph Fourier had so much confidence in the result of his speculations that he applied his method to all of the planets which revolve about the sun. He believed that the determination of the temperature of the poles of the earth would permit him to measure that of the poles, as well as Mars, Jupiter, Saturn, Urania, and, in a word, all the planets which revolve about the sun.

This conception, resting upon mathematical calculations of the first order, led Arago to make strenuous efforts to calculate the probable temperature of the North Pole, and he felt himself warranted in placing the average temperature of that point at from 13° F. below to 22° F. below zero. But it seems that the illustrious successor of Fourier only recognized the minimum, which did not coincide—at least in the northern hemisphere—with the geographical situation of the pole of rotation, and that he would find this to be the case in the Arctic archipelago, where Black observed a temperature of —72.4° F. It therefore seems that Fourier's theory was at fault.

The observations of a temperature of —60° F. brought back to the surface of the earth by *L'Aerophile* as a result of its ascension in March shows that the temperature of celestial space is far less than that which Fourier's theory would suppose. In fact, this temperature has been attained at high noon in an atmosphere where the pressure was still 6.8 in. of mercury, and which consequently would still receive a considerable quantity of heat from the rays of the sun.

Further, at the time Fourier wrote, it was not considered that the thickness of the atmosphere could be considered as being as great as from 112 to 124 miles. Arago took it at less than 37.4 miles, which is only one-quarter of the depth which is ordinarily attributed to it at present.

In the passage where he comments on the result of known observations at his time, Arago adds a few words which might be quoted to the partisans of the absolute zero:

"The heat of celestial space," he says, "whatever may be its intensity, is probably due to the radiation of all the objects of the universe whose light reaches us. Hence it need cause no astonishment," he adds in a note, "when I use the expression of heat in speaking of temperature of from —58° to —76° F.—that is to say, the temperatures which Captains Franklin and Parry have found in their voyages to the polar regions are

really a decrease of heat when we compare them to the hundreds and thousands degrees of cold which would perhaps prevail in the space were it not for the causes cited by Fourier."

Later on, Messrs. Pouillet tried to make a direct determination of the temperature of the celestial medium by employing an actinometer—that is to say a thermometer exposed to zenith radiation. The observation consisted in noting the gradual decrease in temperature observed with a mercury thermometer compared with the temperature of the air taken in the ordinary way. It is unnecessary to repeat the equation obtained by this physicist in this place, but it led him to calculate that —283° F. was the lowest limit of the temperature of the interplanetary space and —224° F. the upper limit. It would be possible to submit these theoretical considerations to a practical verification if we could send testing balloons to make several comparative observations of altitudes ranging from 33,000 to 66,000 ft. in height, and also test the temperature in the shade as well as that under the direct rays of the sun. However this may result, if we admit that at an altitude which does not exceed one-fifth of the depth of the atmosphere we find, in full daylight and in spite of the solar radiations, a temperature which is —58° F., we certainly cannot accuse the preceding figures of being too moderate. In spite of one's self and in spite of the great authority of Biot, one is led to adopt an idea which has been for too long a time held in contempt by astronomers. We admit with this physicist that the constituent elements of the atmosphere cannot retain their form to the upper limits of the atmosphere, as has been taught in all treatises on physics and astronomy.

The excessive cold of the interplanetary space, independent of all conception drawn from the mechanical equivalent of heat, now comes in to play an active *rôle* in the conservation of the materials composing the atmosphere of every planet. The upper regions constitute a reservoir of gaseous materials. In fact, they are occupied by solid, pulverulent, and diaphanous materials, which are probably of very diminutive dimensions, and which, having lost all their elasticity, are retained there by attraction without really weighing anything in the medium which living beings, dwelling upon the surface of the earth, breathe. When, in consequence of circumstances the nature of which is not at all improbable that we shall be able to reach, these frozen particles approach the surface of the planet, they take successively a liquid and then a gaseous form, which is natural and suitable to the circumstances under which we are living.

The transformations which are undergone by the aqueous vapor, and which give birth at first to vapors, then to the cirrus, permit us to gather some idea of those portions where there is an atmosphere of nitrogen and then later an atmosphere of oxygen, as they exist at different heights. In fact, the points of liquefaction of oxygen being much further below zero than nitrogen, the changes of the first gas takes place at greater distances from the surface of the earth. It is very

probable, therefore, that the last modifications of nitrogen occur in an atmosphere of oxygen whose gaseousness has yielded to no attempts on our part, and which exists in a state of absolute purity—at least, no more volatile substance has reached us from the confines of interplanetary space. It is possible to believe, then, that all the hydrogen which is produced by volcanic reaction is gathered here as well as that which escapes from marshes, gases, and our balloons.

A ray of light, therefore, coming from the celestial space, must not only encounter the air which we breathe under the form with which we are familiar, but it is more than probable that it must also pass through unknown substances or progressive solidification of elements which compose the frozen atmosphere of the upper regions. However they may be produced, it is easily understood that they necessarily assume the crystallized form which belongs to them, and which, in spite of the smallness of their absolute dimensions, gives them the power of acting upon the rays of light which come in contact with their facets. It will, therefore, be very imprudent and unscientific to assert that these crystallizations are without any influence on the appearance of the vault of heaven and upon the phenomena which the external surfaces of celestial bodies present, especially in eclipses, where the sun is given credit for many things which are merely the product of illusions.

Astronomy and Astrophysics, in its March number, tells us that the astronomer Pickering has discovered on the surface of the planet Jupiter not only fleecy clouds, which he has recognized and which seem produced by a vapor of a fluid similar to water, but a kind of a gaseous veil formed by a brownish matter the form of which is analogous to our cirrus. Is it impossible for us not to suppose that these appearances are the results of the congelation of one of the elements of the atmosphere of this gigantic world whose presence was seen for the first time in 1892, because for the first time since the stars have been subjected to observations they have taken the pains to carry lenses of great power to the tops of high mountains, and put them in the hands of observers of the greatest skill. Is it not possible that it may be an illusion of the same kind which gives rise to the appearance of the canals of Mars, *apropos* of which large volumes have recently been written? What absurdities would have been wiped out if we could have dreamed of the influence which the works of Messrs. Dewar and Caillet would have upon this most magnificent of sciences, which seems to be a *résumé* of all the others! In fact, as M. Jannsen said, and with great reason, at the end of the conference of 1892, on the application of photography to astronomy: "Astronomy seeks to give us the key at the same time to our past and our future, and possesses the same elevated object as philosophy and theology."

One of the first advantages of the researches made with these exploring balloons would lie in the finding of an unexpected application of the experiments on the liquefaction of gases, one of the marvels of contemporaneous physics, and of

having for its object the establishment of a true theory, which would lead to the study of planetary atmospheres and the constitution of the comets.

In transcribing the results of the experiments of Messrs. Caillet and Dewar, I see that I have committed material error, and that is that nitrogen liquefies or congeals first before the oxygen has lost its gaseous nature. I beg the reader to make the corrections, which I have indicated myself in a later publication.

In considering the properties of the intense cold which exists in the upper atmosphere, we can easily see that it should so act upon the gas of the balloon as to set it free from all the carburets of hydrogen which can in any way diminish its ascensional force. The composition of the mixture at ascension could not be homogeneous. The hydrogen gas which the illuminating gas contained in a considerable proportion should be contained in the top of the balloon, and drive out the former. It is to an automatic action of this kind that I attribute the reduction of the balloon, and the consequent remarkable fall in the barometer to which it gives rise. It is probable that, in consequence of a lack of homogeneity, and in the decantation by the cold, that this balloon contained a mixture which was very close to absolutely pure hydrogen. I am confirmed in this idea by the attempts which have been made to inflate with pure hydrogen taken from a badly constructed apparatus. We were stopped after having put a very small quantity of it—I will estimate it at from 71 to 106 cub. ft.—in the balloon, which, although small, was always sufficient to entirely fill it if it had not been mixed with illuminating gas.

Finally, the practical conclusions to which I am led, and which I would recommend as precautionary for further experiments, are :

1. To cause a current of illuminating gas to pass through a coil which is cooled to a very low temperature, and see if the density is not diminished in considerable proportion. This process might perhaps be recommended to American aeronauts as a practical method of improving their gas, which is very bad.

2. To put some hydrogen gas into a balloon which is comparatively well filled, and into a balloon filled with illuminating gas, the balloons to be of the same cubic capacity, the same construction, and both perfectly impermeable, and see if the registering barometer would not show considerable differences in their densities at different distances from the center of the earth.

3. To make experiments in calm weather, in order that changes in their optical appearances might take place.

4. To try and register in their free ascensions in great altitude with balloons so loaded, and see whether they would give at the upper limit of their ascensions results which were comparable with observations which were made upon them.

5. To place a register on the inside of the balloon and ascer-

tain whether its indications coincide with those of a register placed upon the outside. M. Hermite will try this experiment at the next ascension of one of his exploring balloons; but it would be a very good idea to try it in ordinary ascensions. It could not fail to be interesting for a number of reasons which we cannot lay down here.

6. To attach a large and stiff appendix to the balloon, so that it will come back to the earth while completely inflated, as happened to *L'Aerophile* in consequence of an effect which is very easily explained.

In the ascensional phase the balloon was completely inflated, and in coming down the interior gas gradually contracted, but the exterior air was insufficient to take its place. The people who caught this balloon found it inflated when they got hold of it. If this appendix had been attached it would have been possible to take off the gas gauges and analyze it and to have a proof from a thermometric standpoint of what had taken place. This test, which would be very interesting, can certainly be accomplished.

7. It may be remarked that it is very important that the balloon should preserve its temperature invariable at all times in order to permit the determination of its rectilinear distance from the station by means of a transit or any other means. In this case it would be possible to determine the altitude by means of a simple observation. With two stations there would be a possibility of having means of verification. When ascending, this condition is always fulfilled; it is possible that it might also be accomplished when coming down by taking the precaution which we have already indicated by the use of an appendix. These tests are susceptible of forming other developments, but we limit ourselves to adding that M. Hermite now proposes to get samples of the air of the upper regions. The apparatus which he employs is very original, and different from anything which has been employed up to the present time. Like all other instruments which are intended for taking air automatically, it is somewhat complicated. The following is a note by M. Hermite, wherein he gives some idea of the preparations with which he is engaged:

"The first attempt at explorations of the upper atmosphere was made on March 21 this year by the exploring balloon *L'Aerophile*, which has thrown some light upon certain interesting points. Thus the manipulation of inflation, the complete filling of the aerostat, the doing away with the complication of an automatic unloader, and permitting the ascension to occur regardless of weather; the enormous rapidity of the ascensional movement overcoming the wind, and, as a consequence, the possibility of following the balloon in fine weather almost to the very highest point of its flight, as a kind of sparkling star which is visible in full daylight, and whose height it would be very easy to determine by astronomical measurements; the descent, slow and regular, the recovery of the delicate instruments absolutely intact, are among the

things which have thus far been accomplished, and by which we are going to profit in a new exploration intended to illustrate the problems which were raised by the experiment of March 21. I refer to the congelation of the ink, which interrupted the diagrams; suppression of the thermometer, which ought to have been suspended in the interior of the balloon, and which would have given the temperature of the gas, but was necessary to determine the temperature of the air at a height of 52,500 ft."

In a previous article I made an error in calculation on this subject regarding the difference of temperature which existed in the interior of the balloon, which raised it to an altitude of 8,200 ft. above its zone of equilibrium.

I ought to say that the increase of temperature of the gas ought to have been 252° F.; in reality it was more than double that, or 515° F.

In fact, by using the true formula $D^1 = \dfrac{D}{1 + \alpha T}$, and letting $D = 543$, the weight in grammes of a cubic meter of gas of the ordinary temperature, we will have for the density of D^1 necessary to raise the balloon to a height of from 44,300 ft. to 52,500 ft., the following figures:

$$D^1 = \frac{543}{1 + 286.5 \times 0.003661} = \frac{543}{2.048} = 265 \text{ grammes for the}$$

weight of a cubic meter of illuminating gas at a temperature of 286.5° C. The ascensional force of a cubic meter of this gas will then be 1,293 − 265 = 1,028 grammes, which, multiplied by 113, the volume of the aerostat, would give (116.6 kilogrammes) 257 lbs. as the total ascensional force necessary to carry the balloon up to a height of 52,500 ft. (16,000 meters).

If we admit that the temperature of the gas of the balloon was —4° F. (the same as the air contained in the registering box), we would have for the temperature of the surrounding air, at a height of 16,000 meters (52,500 ft.), the terrible figure of —521.5° F., which is below the absolute zero, as admitted by physicists. I would far rather admit that the temperature of the gas was above that of the box. If a thermometer placed in the balloon had shown —238° F., we would have for the temperature of the surrounding air the —256° F. of which I have spoken. But there is nothing to prove that the temperature of the gas was not above or below this figure. We are in a state of uncertainty in regard to it, and it is necessary that fresh experiments should be made in order to determine what these temperatures actually are.

On the next ascension *L'Aerophile* will be provided with an extremely strong net which will permit the inflation to be performed perfectly regardless of the weather, and the registering apparatus to be suspended in the best possible manner. The thermograph will be suspended in the center of the gaseous sphere, and the barometric thermograph will be located under the balloon in a meteorological screen especially con-

structed to shield the apparatus against isolation. The inscription will be made upon polished and transparent sheets covered with lamp-black.

Special arrangements will be made in order that the thermometers may remain in the shade during the whole of the ascension, and that they will be exposed to a current of air instead of remaining in confined air. *L'Aerophile*, which will be employed in its new test, will only rise to a comparatively low altitude on account of the weight with which it will be loaded, and which will amount this time to about 39.7 lbs.

TEN MILES ABOVE THE EARTH.

DISCUSSION OF W. DE FONVIELLE'S PAPER ON EXPLORATION OF THE UPPER ATMOSPHERE, BY H. A. HAZEN.

THE preceding paper describes one of the most important physical investigations ever undertaken, and, considering the extreme difficulties of such a beginning, we must regard the research as wonderfully successful. On page 379 there is a table of the observations reduced from the traces of the barograph and thermograph. This table is very suggestive and instructive, and will bear any amount of study. We cannot fully accept all the deductions made by M. Hermite, although it may be impossible to explain all the anomalies. It is hardly probable that solar radiation at the highest point has produced anything like the results estimated by the author. We see from the table that the temperature apparently began to rise between 12.45 and 12.50, and at 12.57 the temperature indicated was only —52°, while it had been —60°, and the temperature required to freeze the ink was —67°. Even allowing for a slightly greater shading of the ink than of the thermograph bulb, I still think the ink could hardly have frozen. Again, the barograph trace did not begin till 14.30, when the indicated temperature was —6° or thereabout, and the thermograph trace did not begin for 95 minutes afterward. These facts seem to show that it was hardly the freezing of the ink that caused these anomalies. It may be that the pens did not touch the paper uniformly. As to this enormous radiation, it should be noted that, at very low temperatures, the thermograph bulb is very liable to have *freaks*, and when we add to this the probable effect of the diminution of air pressure upon the bulb, we see how uncertain these indications may become. It is of the extremest importance that the *bulb* of this instrument be immersed in a bath at —60° to —80° F., and a test be made of its behavior, also it should be placed under the air pump to test the effect of a 4 to 6 in. vacuum.

In like manner the effect of *changes* in temperature upon the vacuum box of the barograph should be carefully determined, especially at very low temperatures and very low pressures.

The most interesting computation of all is that relating to the probable temperature of the highest point reached (10 miles) at 14.30. It is suggested that the equilibrium point for the balloon would be reached at about 43,000 ft.—that is, if the gas and air temperature were the same. Now, since the balloon reached a point 9,000 ft. higher, it must have done so only from a heating of the gas from intense solar radiation to a temperature 252° higher than the surrounding air. This would give the air temperature as 256° below zero! There are serious difficulties in this view, however. By examining the table we see that the diminution of temperature with height obeyed a very regular law, and, continuing this law up to the highest point, it would make the lowest temperature —104°. It is very doubtful, however, if the balloon ever reached this height, as the barograph record was probably vitiated by the temperature effect. The balloon would radiate its heat to the sky from a hemisphere, and it would conduct its heat to the air at an enormously lower temperature over the whole sphere; it is very doubtful if the temperature of the gas was much more than 20° to 30° above that of the air. More than this, theory indicates (I do not accept it, however) that, in the enormous expansion of the gas to six times its bulk, it would be cooled 225° F. If this be granted, it is probable the temperature of the gas would be very much lower than the outside air, and this computation of M. Hermite's would not hold. Glaisher found in his ascensions up to 30,000 ft. that the temperature above 10,000 ft. diminished at the rate of about 1° F. in 1,000 ft., instead of 1° in 300 ft., which was the law below that height. Of course England has an insular climate, and this may account for a part of the difference. It is probable, however, that under ordinary circumstances the diminution of temperature with height is nothing like as great as even this 1° in 313 ft. determined from these records. It is quite doubtful if the actual temperature at the highest point reached by the balloon was lower than —70° F.

There is still another consideration which should have a good deal of weight. M. Hermite makes no allowance for the temperature of the air column in computing the heights, but it is well known that this is an extremely important point. The temperature correction, if —104° be adopted for the highest point, would be —7,100 ft., and if —256°, as computed by M. Hermite, be adopted, the temperature correction would be —15,000 ft., so that the elevation of the highest point would not be more than 40,000 ft. according to strict computation. I do not make these remarks to throw discredit upon this work, but to show the wonderful interest aroused by this single ascension, and the extraordinarily complex nature of the record brought back.

It should be noted that nearly the same sort of a record was

obtained upon September 27 with this same *L'Aerophile*, at Paris—that is, the pens left the paper at the height where the temperature and pressure were very low. The second ascension was near the center of a storm, and showed a diminution of temperature of 1° F. in 376 ft. This enables a most interesting comparison with the temperature observed in March in the center of a high area of pressure. We see that in the high area the relative diminution of temperature showed a rapid increase of cold, while in the storm there was a much slower increase. This was to be expected, though directly contrary to the views of some meteorologists.

It is very much to be hoped that this research may be stimulated, and that we may gain a knowledge of these conditions in summer and winter and under varying meteorological factors. It will be useless to attempt to obtain a perfect knowledge of average storms, hot waves, cold waves, etc., such as we experience in this country, for they do not have them abroad. The cold experienced there is due to radiation, and the storms and high areas are practically a stationary phenomenon. This should stimulate the investigation in this country, where we have these conditions to perfection.

Not the least interesting part of this study in Paris was the distance (77.7 miles) to which the balloon reached in 6 hours, 46 minutes. This was at the rate of about 12 miles per hour. It is very doubtful whether at a similar height in this country the distance would be less than 300 or 400 miles, except possibly in the center of a high area. Observations on high mountains in Europe show a tendency to rather low wind velocities, and to a motion either from southwest or back again from northeast. In this country there is a remarkable uniformity of the higher currents from the west. It would be difficult to estimate the value of the results brought back by a few such balloon trips in this country.

H. A. Hazen.

December 23, 1893.

APPENDIX A.

DISCUSSIONS OF PAPER ON THE MECHANICS OF FLIGHT AND ASPIRATION,* BY A. M. WELLINGTON, M. AM. SOC. C. E.

BY J. BRETONNIÈRE.

I HAVE read with a great deal of interest the paper in which Mr. Wellington makes a *résumé* in 12 propositions of the theory of flight. The author holds our attention by his clear, concise and accurate style. I agree with what he announces regarding known facts. I am also disposed to agree to his other propositions. I am thus in accordance with the author on 11 propositions, but in regard to the twelfth, relative to aspiration, which we ordinarily call soaring, we separate.

According to Mr. Wellington the bird, in order to reach a height, has merely to glide along in the direction of the wind; then, when it shall have reached the speed of the aerial current, increased by that obtained by its own gliding motion, it makes a return and comes up against the wind. This opinion is announced with the clearness which characterizes the whole of Mr. Wellington's paper. According to him it is a matter of indifference whether the turn back is made suddenly or gradually, and if the bird has recourse to the circular mode of soaring, it is merely that it is more graceful.

It can hardly be that it is in order to obtain greater grace of form that the bird passes through circular trajectory during the soaring portion of its flight, but because this portion of its trajectory includes a transverse gliding motion. No; the gliding motion in the direction of the wind, followed by a sudden return back, cannot give a bird a gain in height; and furthermore, as I should say in discussing the article appearing in AERONAUTICS for October, 1893, by Mr. Winston, the bird which can support itself upon the air, while flying before it, has no means of utilizing all of its *vis viva*. The bird cannot. after having plunged through the air, return with a speed greater than $V - 2v$, relative to the earth, V being the total speed of the bird and v that of the wind. Let us see by means of this formula whether the bird can successively use a gliding motion with the wind in order to gain in height when turning suddenly against it. Let v_1 be a speed acquired by gliding, so that $V = v + v_1$. In order to obtain the speed v_1 the bird

* See page 227.

would lose a height equal to $h_1 = \frac{v_1^2}{2g}$. If we suppose the momentum acquired, the bird will turn back with the speed $V - 2v$, where, substituting V by its value in speed, $(v + v_1) - 2v = v_1 - v$.

It will plunge through the air, the velocity of which is v_1, with a speed $(v_1 - v) + v = v_1$. The momentum will give it a gain in height equal to $\frac{v_1^2}{2g}$; that is to say, equal to the loss of height which it shall have dropped during its gliding motion. By such a series of manœuvres the bird would theoretically suffer neither a gain nor loss of height.

Let us apply the preceding to the case considered by Mr. Wellington in his paper, of a bird having acquired a speed of 20 miles per hour in a wind blowing 20 miles per hour, and having consequently an actual velocity relatively to the earth of 40 miles per hour. The bird will have acquired its gliding speed at the expense of a loss of height 13.38 ft.

After its blow against the wind it will have a backward movement of a velocity $40 - (2 \times 20) = 0$ miles. It will come up against the wind with an actual velocity of $0 + 20 = 20$ miles, which will give it a theoretical gain in height 13.38 ft.; whence there is absolutely no gain as the result of the whole manœuvre.

Suppose the gliding speed to be something else than 40 miles, for example, then the height acquired on the return will theoretically be nothing else than that which was lost, with no gain.

To the objection which we have just made we will add that Mr. Wellington estimates the height which a bird could reach on turning against the wind at 53.51 ft. This is, in fact, a height corresponding to the momentum at a speed of 40 miles.

It is thus with a speed of $v + v_1$, and not with that of v_1, which we admit that the bird, according to Mr. Wellington, plunges into the air. Mr. Wellington does not seem to, and doubtless does not, believe in the same conception of gliding as Mr. Winston; yet admitting the above formula of $v + v_1$ will lead to the adoption of the theory of the latter. In fact, if the gliding speed v_1 decreases until it is reduced to 0, the quantity $v + v_1$ is reduced to the speed of the wind, and the bird can then only glide at the speed of the wind, and can thus turn up against it with the speed v and rise to a height equal to $\frac{v^2}{2g}$. The latter results, I will not say are from the formula of Mr. Winston, since he gives no formula, but from what appears to me to be his real idea.

In his conclusions Mr. Wellington turns his attention to a flying machine that is still to be invented. My ideas are not always in accord with his. There is one point in particular upon which I do not look as he does, and that is the *rôle* of

the motor. Mr. Wellington seems to wish to reduce it to a very insignificant affair. I, on the contrary, consider it as a very essential feature. What would become of the machine without the motor when floating in midst of space or in a very heavy wind? When would it be possible to do the rising and landing without its aid?

The flying machine or aerodrome of the future will, we think, have a fixed carrying surface provided with a motor and a screw on a vertical shaft. Our imagination has not yet conceived of the exact form of carrying surface. Will it be flat or like the wing of the bird? Will it be composed of a single piece or several pieces? My opinion is that it can be made in one of several ways: experience will show the best. As for the screw, a double one would probably be best in order to prevent the flying machine from turning around. It remains to be determined whether the two screws should turn together concentrically in opposite directions, or be placed at two points on the machine. Our double screws should be composed of flat planes of metal with very slight inclinations to their arms, and should turn with great speed. They should only be driven at intervals, the inclined surface serving, as we have said while at rest, as simple carrying surfaces.

The screws while at work ought to be able to raise the aerodrome at a considerable speed. It will then serve to raise it vertically at the commencement of flight, and to lower it slowly on making the landing. Its assistance will be very useful immediately before flight and at landing, in order to give the flying machine a proper adjustment in spite of atmospheric disturbances, and this can be done by a reversal of the machine. While in full flight it could aid, if it was considered desirable, in the handling of the aeroplane by raising the aerodrome in its motion against the wind. In times of calm the aerodrome could lay out its course in a practically straight line on a plan composed alternately of glidings and motion against the air, in which the screw would serve to raise the apparatus during the second part of the manœuvre. While making a run in a straight line with the wind blowing the services of the screw would be the same. Our flying machine, like the soaring bird, will owe its speed to gravity, but at the expense of a certain height lost. This height will be gained by a motion against the wind, like the soaring bird, and all by the action of the screw. There is, therefore, a certain analogy between the soaring of the bird and the operation which we have pictured for our aerodrome; but the resemblance is not complete, for the wings of the bird ordinarily give it a horizontal motion, while the propulsion of the screw is in a vertical direction

In the early attempts the relative uncertainties as to the best form will show themselves and develop the best conditions of equilibrium which will be most favorable to our screw. With it the inexperienced aviator should only raise himself at first a few feet above the ground and then descend; raise himself higher and then come down again; attempt a little gliding if

everything has worked well up to that point, and then adjust his apparatus as experience may indicate to be necessary, and thus get on gradually until he has definitely obtained possession of the Empire of the Air.

BY H. A. HAZEN.

Mr. Wellington read a paper on this subject at the Aerial Navigation Congress at Chicago, and this has been published in *Engineering News* and on pages 227–240. This article has caused a great deal of discussion, and, it seems to me, is of great importance at this day when the air is so full of theories regarding flight.

Is there not a serious error in some of the computations in the original paper on page 234? For example, it is stated in the hulk, inclined plane and ball problem, that, if the ball should be dropped vertically, "it would strike the ground (or deck) with the velocity due to its original energy plus 13.38 ft. more of fall," giving a velocity 28.28 miles per hour. A little earlier in the paper it is stated that if the ball should roll down the inclined plane it would attain a velocity of 40 miles per hour, as regards the earth, made up as follows: 20 miles per hour initial velocity, and 20 miles per hour acquired in falling the 13.38 ft. It seems as though there must be a fallacy here. Certainly the rolling down the inclined plane would retard the ball vastly more than falling through the air the same vertical distance. If in falling vertically 13.38 ft. the ball acquires an additional velocity of 8.28 miles per hour, it must acquire less than that in rolling down the inclined plane. As to an impulse being given to the ball from the motion of the inclined plane moving at a velocity equal to the velocity of the ball at starting, it would seem that this additional impulse is entirely imaginary, and, moreover, such additional impulse is not allowed for in the computation, for, as we have just seen, the 40 miles per hour at the foot of the plane is made up of two quantities, the initial velocity and the fall, and into it this impulse does not enter.

One important consideration in this discussion relates to the resistance of the air upon the bird. It seems very evident that, when the bird soars with the wind, the force of the current in moving it must be exceedingly slight; even if the bird should come to a dead stop with its wings spread in soaring the current would have very little effect upon it in starting it up. If the bird were moving with the current and having its velocity, it is evident this effect would be nothing. There must be a very serious fallacy in a computation which will carry a bird more than 26 ft. higher than its starting-point by simply dropping 13 ft. This is beating perpetual motion and a good deal to spare.

Suppose the bird with an initial velocity of 20 miles per hour should suddenly strike a stationary current, are we to suppose for a moment that it would be carried 13.38 ft. upward, no matter at what angle it held its wings? The re-

sistance of the air, all the greater because the bird mounts it at an angle, would retard it greatly. More than this, when the bird reached the highest point to which it could climb, it would be in still air and would at once begin falling, as agreed to by all, unless it put forth an effort to sustain itself. Suppose now, instead of mounting into still air, it wheels and mounts into a current which resists it at the velocity of 20 miles per hour, the resistance would be much greater than in the previous case, and before it had mounted many feet it would lose its own velocity and would fall again unless it exerted itself. It seems to me it is of the extremest importance to bear in mind that the impelling power of the air upon a bird soaring in it and at its velocity is nothing, and also this same impelling power is exceedingly slight in the case of a bird having no velocity and trying to soar.

The argument that because we do not *see* a bird use its wings in supporting itself, there must be some other source of energy acting against gravity, and that source is one which manifestly goes against mechanical principles, is certainly of very doubtful utility. I have noticed in balloon voyages up to heights of 16,000 ft. that the air in any stratum is continually changing its velocity just as at the earth's surface, where it is not uncommon for the wind to drop off one-half in its velocity in a few seconds. I do not refer to a change of velocity in different strata, but in the same stratum. Why may not this have some influence upon the soaring bird? As to the effect of a sudden turn upon the bird, I witnessed the flight of a buzzard under rather remarkable circumstances once. The buzzard was very nearly 500 ft. above my head, when it suddenly turned at an angle and shot to the earth in an inclined path, striking the ground at about 1,000 ft. from where I stood. The bird did not seem to be injured. The time of this descent seemed exceedingly short. I hardly think it could have been more than one minute, and possibly it was only a half-minute.

APPENDIX B.

DISCUSSION OF THE THEORY OF THE AEROPLANE AS CONTAINED IN THE PAPER ON THE INTERNAL WORK OF THE WIND,* BY PROFESSOR S. P. LANGLEY, AND READ BEFORE THE CONFERENCE ON AERIAL NAVIGATION, AT CHICAGO, AUGUST, 1893.

BY J. BRETONNIÈRE.

IN his paper on the "Internal Work of the Wind," Professor Langley has described some observations which he has made upon the velocity of the wind, and has deduced therefrom various considerations and theories, according to which a flat light body, such as a bird or a flying machine of the aeroplane type, as built by man, would be able to rise in the air while going directly before the wind, without any other manœuvring than that of presenting itself at a proper inclination to the aerial current.

In what follows I propose to ascertain whether the facts as stated, and the reasons as presented by Mr. Langley, prove that such simple means can give results of such wide importance. This is really a matter of very great importance, for the bird or aeroplane (for the sake of simplicity I shall hereafter merely consider the bird) could, when he has attained a certain height, glide in whatsoever direction he pleases (at least in winds which are not too violent), and would thus have the faculty, without resorting to spirals and other complicated manœuvres, to travel at will through the air, provided that the wind was sufficiently strong—a reservation which is not without importance.

In his introduction Mr. Langley shows that the aeroplane, in the form of a bird, which is 100 times heavier than the air, maintains itself in space, without any flapping of wings, upon an invisible medium, and has been the object as yet of no satisfactory explanation. He would not offer any himself if he confines himself to an ornithological problem; but in his eyes the question is of the mechanical order, and one of great utility. After having said that Mr. Mouillard claims that under certain circumstances the bird, without any flapping wings, rises and actually advances against the wind, he describes the performance of the aeroplane, which he has witnessed in the suburbs of Washington. These performances are spirals: the observer remarks in passing that a bird rarely flies in a straight line.

* See p. 66.

Mr. Langley pays particular attention to the problem of the aeroplane, since an accidental fact brought the desirability of a solution to his attention. While making some experiments with his turning table at the Alleghany Observatory, he remarked that the register, which showed the speed of the wind, indicated great irregularities. This fact appeared very important to him. He thought that the internal working of the wind might furnish an effort capable of raising a bird, and he decided to carefully observe it. According to the author, the common notion of the wind contains an element error. The wind is not a homogeneous current, having approximately a uniform velocity. It is, on the other hand, subjected to complex phenomena that are little known.

Mr. Langley undertook the study of the wind with light anemometers, which inscribed their registration on a chronograph by means of an electric wire. The results thus obtained in winds of different velocities have given him the data for constructing diagrams, some of which are reproduced with the author's paper. These diagrams graphically show the variations in the speed of the wind, in which the abscissæ represent time, and the ordinates the velocity of the wind.

Mr. Langley has connected the successive points which were obtained in his observations by straight lines, but this indicated speed had to be considered as being almost the same a few seconds before and after the point noted. Before taking up with Mr. Langley the applications which he has undertaken in his paper, I wish to recapitulate the remarks which have appealed to me in that portion of his work which I am about to analyze.

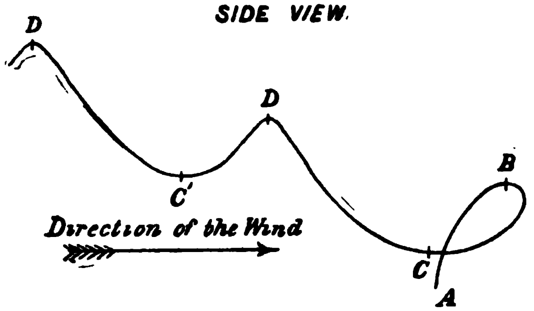
Mr. Langley thinks that no satisfactory explanation of the question which he has under consideration has yet been given. It is the natural opinion of an author who approaches a problem of any kind with a new solution. I will return later to the observations of Mr. Mouillard, who is cited by the author. As to the hypothesis which Mr. Langely regards as erroneous, according to which all writers up to the present time have considered the wind as a homogeneous current with a uniform velocity, it is in my eyes not a hypothesis of kind, but a hypothesis of form—that is to say, the writers in question, of whom I am one, know very well that in the wind the speed varies at every instant ; and that if in their calculations they consider it as uniform, at least for a few moments, it is that they have not found any better means of approaching the problems which they study. We will soon see that Mr. Langley himself returns to the same hypothesis.

Let us now pass to the chapter of applications. I expected to see that the diagrams which Mr. Langley has produced would serve for these applications, but my expectations were not fulfilled. Mr. Langley has not only failed to maintain his point, but he has made one remark which seems to take away all the signification of the work which he has evidently executed with so much care. He declares that it should be remarked in passing, while it seems to me that it

should be an object of investigation, that the velocities indicated on the diagrams are the horizontal velocities of a straight current passing over the anemometer. Then he says that if the soaring bird was located where the anemometer was placed, a point of its surface would have been sustained by the velocity indicated by the instrument, while the other points of his wings would have been under the action of entirely different velocities. Yes, without doubt, if I thoroughly understand the views of the author; but if such a state of affairs exists for the surface of a bird, what are we to expect for that of a large flying machine which will some day carry a man in space? Such an avowal throws doubt into the mind of the reader, who is led to ask whether any valuable deductions can be drawn from Mr. Langley's diagrams. The author himself considers the movements of the air to be so complex that no diagram can accurately represent them. Whether this last opinion of Mr. Langley be well founded or not, I cannot accept the different hypotheses which he then puts forth, and one of which shows the wind to be composed of an average current and of counter currents.

As examples of application, Mr. Langley gives three cases in which he shows the bird manœuvring and rising to some height. I cannot follow the author in the deductions which he develops, since, in admitting a wind of a uniform velocity during the period under consideration, I am led to believe that Mr. Langley is himself placed at a standpoint which permits him to see that he is thus brought back, as I announced above, to the method which he had already declared erroneous. The examples given do not correspond at all with the diagrams, and Mr. Langley avows that he has intentionally exaggerated the circumstances. I freely admit that in the case which the author has cited—that is to say, under these exaggerated conditions—that a bird will rise; but where I cannot accept the opinion of Mr. Langley, is when he says that the same general effect will be obtained under conditions when the anemometer observations were shown to be realized in nature, which is to say that the conditions corresponding to his diagrams actually exist. I certainly may be permitted to express my astonishment at hearing the author now invoke the diagrams which he has just declared to be incapable of representing the realities of the case. Nevertheless, these diagrams falsify those facts which Professor Langley brings into his observations, yet I do not believe one ought, in spite of the slight value which their author has been able to attribute to them, for an instant refuse to admit them into the discussion. But how can we interpret them? Doubtless, as Mr. Langley himself has done, as he said above, by supposing that for each point in the diagram the speed corresponding to this point was the same for a few seconds before and a few seconds after the passage of the said point. The last three examples offered by Mr. Langley are particularly developed by him. He devotes himself to a bird moving in a direction opposite to that of the wind, which wind is itself composed of gusts and calms.

Let us examine in detail whether the results obtained, according to Professor Langley and also according to myself, of the problem which he has laid down will be the same under the conditions which correspond to the diagrams. I have borrowed from Mr. Langley his premises and the figure which he has drawn for the trajectory of the bird.



The bird started at A, where he was at rest in a gust of wind, with a velocity of v, by which he will be able to raise himself to the height $h = v^2 \div 2\,g$, the generatrix of the velocity v. At the instant it reaches the point B, having reached the height h, there comes the period of calm. He then glides with a continually increasing velocity until C is reached. He will thus descend by the height h_1, which will have given him the velocity $v_1 = \sqrt{2\,g\,h_1}$. Then comes another gust with a velocity of v. The bird will with this have the power of rising to a height $H = (v = \sqrt{2\,g\,h_1})^2 \div 2\,g$. When he reaches D he will then have exhausted this power, and will have touched a new period of calm, during which he again glides down the height h_1 until he reaches C'. Then a new gust comes during which he will again rise to a height $H = (v + \sqrt{.2\,g\,h_1})^2 \div 2\,g$ until he reaches D', and then through a succession of gusts and calms will rise with the same regularity. In trajectory $A\ B\ O\ D,\ C'\ D'$, the part which it is especially necessary to notice is that the $D\,C'\,D'$, which should reproduce the portions $A\,B\,C\,D$, which is nothing more than an introduction. In the run $D\,C'\,D'$ the bird has made a gain in height equal to $H - h_1 = (v = \sqrt{2\,g\,h_1}) \div 2\,g - h\,h_1 = (v^2 + 2\sqrt{2\,g\,h_1}\,v) \div 2\,g$. This value is never negative. It increases or decreases with v and with h_1. For $h = 0$, it becomes $v^2 \div 2\,g$, representing the height which the gust would give to the bird starting from a state of rest. For $v = 0$, it becomes zero—that is to say, there is neither gain nor loss, the height of the fall having been regained. Ex-

pressed theoretically, the bird in the trajectory $D\ C'\ D'$ will always gain in height, or at least would never lose.

In order that such results may be obtained, it has been necessary, first, that the succession of periods of calm and gusts should be periodical; second, that there should be no loss in mechanical effect.

If the gust is not on time—that is to say, if any interval should intervene after arrival of the bird at the point C', beyond which the fall would continue at an increased velocity, he would be compelled, until the wind reached him, to glide on, losing from 10 to 20 per cent. in height of the distance traversed. If the gust continues when the bird has passed the point D', where all the height which he could have acquired shall have been attained, he would again be compelled, in order to sustain himself, to glide on at a pure loss. Now these losses in height do not exist in the exaggerated case given by Mr. Langley, since the wind and calm always arrive at the proper time. On the other hand, according to the diagrams of Mr. Langley, the losses in height will be numerous. There is, therefore, no standing point between the two situations, and this reason would be sufficient in itself to condemn the application to the second situation of the results admitted for the first.

As to the losses of efficiency, they are inevitable, as well in an aeroplane as in any other kind of mechanical work. Let us try to ascertain their influence in the question which is attracting our attention, by a comparison of an exaggerated case with one which can be actually found in nature. In order to study this subject exactly, it is necessary for us to know the coefficient of efficiency for the work of an aeroplane, or rather the coefficients, for, in my opinion, each form of bird ought to correspond to a particular coefficient. In default of this data we must content ourselves with an approximate solution by adopting an approximate coefficient. Let us take 0.80, which is doubtless very high for most machines, but which under present circumstances the simplicity of the aeroplane will warrant us in assuming.

If, now, we admit that the bird can only utilize 80 per cent. of its active power, it will have, when it reaches C', a velocity of only $\sqrt{0.80 \times 2 g h_1}$, instead of $\sqrt{2 g h_1}$. When it shall have reached D' instead of $v + \sqrt{2 g h_1} \div 2 g$, gained as the result of the gust and of the previously acquired velocity, it will have $0.80 (v + \sqrt{0.80 \times 2 g h_1})^2 \div 2 g - h$. When $v = 0$, this last value is reduced to $0.80^2 h_1 - h_1$, which is a negative quantity.

The following table has been drawn up in order to permit us to make a comparison for the trajectory $D\ C'\ D'$, of the gains in height to be attributed to a bird in actual flight, with a given coefficient of efficiency equal to 0.80, where the gain in height is purely theoretical. This brings out in relief an important fact in the discussion. While in the upper part of the figure the gains in height, in spite of the loss of efficiency, re-

main considerable, in the lower portion, on the other hand, where speeds are less than 5 meters per second, or from 10 to 12 miles an hour for the velocity of the wind, they become negative, or are reduced to figures too insignificant to compensate for the slight loss in height due to lack of opportunity.

Velocity of the Gust per Second, v.	Height of Fall of the Bird, h_1.	Theoretical Gain in Height, $\frac{(v + \sqrt{2 g h_1})^2}{2 g} - h_1$	Gain in Height, with a Coefficient of Efficiency of 0.80. $\frac{0.80 (v + \sqrt{0.80 \times 2 g h_1})^2}{2 g} - h_1$
Meters.	Meters.	Meters.	Meters.
20	20	60.78	37.71
	10	48.92	32.90
	5	40.57	28.84
	2	33.09	24.62
10	20	25.26	11.11
	10	19.36	10.57
	5	15.18	9.43
	2	11.44	7.86
5	20	11.35	0.86
	10	8.40	2.43
	5	6.32	2.78
	2	4.44	2.54
2	20	4.22	—4.30
	10	3.05	—1.47
	5	2.22	—0.23
	2	1.46	0.32

Now, on the other hand, it is manifestly in the upper portions of the table that it is necessary to place the exaggerated instances of Mr. Langley, while, on the other side, within the 10 and 12 miles an hour, are comprised almost all the variations of speed shown in his diagrams. Mr. Langley has, therefore, been in great error from the standpoint of losses of efficiency, in attributing to those cases, which are found in the lower part of the table, results which must necessarily belong to cases properly located in the upper part. There is no doubt but that a more accurate coefficient of efficiency would give more accurate figures for the gains in height, and the upper limit of 5 meters per second for the speed of the wind might rise or fall; but in the eyes of those readers who have taken the pains to follow the formation of the table, there will always exist a lower portion, which includes a great part of the variation of speed noted on the diagrams, and always leading to the same conclusion.

Finally, Mr. Langley gives as a reason for the action of an aeroplane the variations in the velocity of the wind. In order to study this question he has carefully drawn up and represented by the diagrams shown in the different illustrations those variations which have been recognized before, but not accurately observed. Having reached this point in his examination, instead of using his diagrams directly for his dem-

onstrations, he had recourse to cases which he himself declared were exaggerated, and then claims that the general result thus obtained in these exaggerated cases is applicable to the fixed conditions in nature. I have shown that, on the one hand, the lack of opportunity presents itself very often in nature, and assuming the premises, which Mr. Langley calls exaggerated cases, on the other side, the losses due to lack of efficiency would absorb all, or almost all, of the mechanical effect to be expected from the greater portion of the pulsations indicated in the diagrams of the author, and will thus prevent them from being accepted as conclusive. Mr. Langley's theory is thus reduced to a simple hypothesis of which he has no proof.

By what possible actual performance of an aeroplane is Mr. Langley authorized in considering it possible that a progressive ascending flight can take place against a horizontal wind, along a trajectory represented in plan by a straight line? Mr. Langley has made some observations himself; he has especially seen that circles are traced, the bird moving, he says, very rarely in a straight line. He appears to attach particular value to an observation of Mr. Mouillard, that under certain circumstances the bird rises without flapping its wings, and actually advances against the wind. This is hardly accurate; he has failed to point out the circumstances. I myself, in one of the countries where Mr. Mouillard made his observations, have seen birds rise by advancing against the wind; but it was sometimes under the influence of an ascending current, sometimes by natural gusts caused by an inequality in the surface at the point; but never have I seen it under the circumstances cited by Mr. Langley—that is to say, full in the face of a horizontal wind, and in straight lines taken on the plan. Has Mr. Mouillard observed some things that have escaped me? Perhaps so; but Mr. Langley cites nothing in this regard which can teach me anything. However that may be, the aeroplane flight, as made by a bird, is especially composed of curves and broken lines, so that I am still right in saying, *if Mr. Langley's theory was well founded, the soaring bird would simply have to be alive; but having at his disposal simple and efficient means of arriving at his object, he almost always employs complicated means.*

CONSTANTINE, ALGERIA, June 25, 1894.

APPENDIX C.

REPLY OF MR. BRETONNIÈRE TO CRITICISMS UPON HIS THEORY OF SAILING FLIGHT.

1. *Answer to Mr. Chanute's Discussion.**

THE general criticism of Mr. Chanute to my theory is that it is incomplete. He says that I do not demonstrate by calculations that the sailing bird must advance against the wind as well as rise; that I have not taken into account the varying shapes of birds, and that I do not explain why certain species of birds can sail and certain others cannot.

These observations of Mr. Chanute are just; but was it possible for me, is it possible for any one, in the present state of knowledge, to comply with these desiderata? We know, through the labors of several authors, notably those of Professor Langley, the action of air upon *plane* surfaces; but we know next to nothing concerning other surfaces. Mr. Lilienthal quite recently expressed the regret that Professor Langley had not experimented upon wing-shaped surfaces, but these vary so much that the learned professor would have had a most complicated task if he had undertaken it. The authors who have hitherto attempted to calculate the reactions under the wings of birds have been compelled to consider them as planes, with a coefficient introduced; which method, they are the first to acknowledge, is not accurate. The resistances of the bird's body and wing edges likewise require the application of approximate coefficients. If such calculations in a general discussion partially satisfy the reader, would he accept them to determine so delicate a question as the exact point where the shape of the bird designates whether or not he can sail? Moreover, this problem involves still other difficulties, and long, arduous investigations remain for our successors.

Content with the consideration of general results, applicable to both geometrical aeroplanes and to bird shapes (which latter are approximate aeroplanes), I have included the resistance of the bird's body and wings in the general loss of effect, which also includes the transformations of energy, etc. The loss of effect due to the wing edges and body may be such that the bird will regain less altitude when facing the wind than he has lost in gaining speed, and he can then glide only intermittently, and thus will not be a true sailing bird. It is

* See p. 201.

thus that I account in a general way for the fact that certain birds can sail and certain others cannot, but it is not possible, as above explained, to establish the precise mechanical limit between the sailing and the gliding bird. Moreover, it sometimes happens that ornithological rather than mechanical reasons determine whether a bird shall soar more or less. Birds, for instance, who are expert sailers frequently beat their wings, because nature, having made them birds of prey, has added to their sailing endowment that of alert wing flapping in the hunt. In fine, I may add that many birds which cannot be classed as sailers can soar upon occasion, as witness the domestic pigeons which I have described in my memoir as tracing orbits in the air.

If I have not demonstrated by calculation that the sailing bird can, while rising, advance against the wind, I have established by observation that he has the ability to do so, by means of orbits and zigzags in a horizontal wind neither too feeble nor too violent. In point of fact he does not always need to practice this manœuvre, and when he does it may be far from us, after having performed under our eyes the movements merely designed to gain height, for we cannot hope to see the bird display all his talents at each instant. If, however, we observe cases when the performance is necessary, and within the range of vision; such, for instance, as those in which the bird returns to his nest against a head wind across an open space, we shall see him either advancing against the wind by a series of zigzags, gaining a little altitude at each zigzag, or else confiding himself to the wind, as it were, perform a series of orbits which will bring him back to straight gliding at a great height above the point where the orbits began. There is a minor point in which I do not agree with Mr. Chanute, where that author says that I add the vertical velocity of the bird with the horizontal speed of the wind. The bird's velocity, when I add it to that of the wind, may sometimes be vertical, but it is oftener inclined and more nearly horizontal than vertical. It is true, however, that there is a corresponding loss of altitude, and that, as Mr. Chanute says, there is a loss of effect in transforming the direction of motion. Upon this point, therefore, our disagreement is slight.

2. *Answer to Mr. A. F. Zahm's Discussion.**

As well apprehended by Mr. Zahm, it is my opinion that the bird can sail on rigid wings in a horizontal wind of uniform speed and direction. According to Mr. Zahm, "this is equivalent to saying that the bird could soar inside an indefinitely large closed car moving with uniform speed on a straight level track; and this in turn is equivalent to saying that a ball, starting from rest at a certain point inside such a car, could roll down a properly formed groove and rise above its initial level."

* Published in AERONAUTICS for June, 1894.

I will first remark that, as a general thing, it is unsafe to solve another problem instead of the original one, unless the similitude be perfectly evident or can be proved to be exact. The author resorting to this method may reach fallacies by neglecting some important differences. I have noticed, in discussing the theory of Professor Langley of soaring flight, a mistake of this kind.

In this instance I do not admit that the problems stated are identical. Why does Mr. Zahm complicate the question by introducing this large car, upon whose mass and upon whose sides reactions may occur which do not exist in the natural problem? Even were these reactions the only counter-arguments to present, they would invalidate the affirmation of Mr. Zahm as to the identity of the situations.

Mr. Zahm states that sailing flight is impossible in a uniform wind, but he gives no reason why. He might, it seems to me, have briefly indicated the deficiency in my conception of the results of the manœuvre which I call the "relative squall," which may be performed in a uniform horizontal wind, and which is, so to speak, the foundation of my theory. Does Mr. Zahm hold that it is impossible for the bird to acquire, in a horizontal uniform wind, speed in a transverse direction, either through initial velocity, or in the course of circling, or by a zigzag? Or does he hold that the bird in possession of this transversal speed cannot then head against the wind so as to breast the current, with an upward angle of incidence, and a speed due to the sum of his initial velocity and that of the wind less the loss of effect? Let Mr. Zahm establish that either of those two points is a fallacy, and my theory will be disproved, but I object to the introduction of the large railway car into the question.

APPENDIX D.

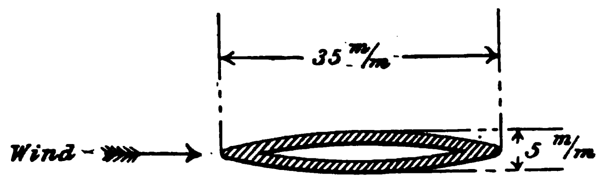
A THEORY OF SAILING FLIGHT AND AEROPLANES.

REPLY OF MR. KRESS TO MR. CHANUTE'S DISCUSSION ON BRETONNIÈRE'S PAPER ON SAILING FLIGHT.*

MR. CHANUTE, the esteemed author of one of the most excellent and complete aeronautical books of to-day, to wit, "Progress in Flying Machines," has thought the modest papers which I sent to the Conference on Aerial Navigation in Chicago worthy of his discussion, and has courteously indicated those points which seemed to him doubtful or obscure. I will endeavor to resolve those doubts and obscurities as briefly as possible.

In the discussion of Mr. Bretonnière's paper on Sailing Flight, Mr. Chanute also mentions my article among others, and says: "Mr. Kress, who argues that the bird gathers energy by passing from one stratum of air into another stratum blowing at greater speed, but who furnishes no evidence that air is usually stratified in that peculiar way;" and further, "Even if it were in regular strata, instead of the irregular pulsations shown by the diagrams of Professor Langley."

It will perhaps be best to copy here the sentences of my article which were called in question in the excellent English translation: "There are *streaks* and *waves* of air of different speeds, generally increasing in velocity with the altitude;" and further, "It must be *particularly* noticed that the more rapid currents of air *need not* necessarily be above each other."



The assertion is nowhere to be found in my article that the different velocities of the air must be arranged the one above the other in sharply defined layers; on the contrary, I assume

* See p. 201.

it as self-evident that the differences in wind velocity are quite irregular in their speeds and direction, so that my theory does not conflict at all with Professor Langley's diagrams, and these irregularities in the wind velocity cannot have any influence on the correctness of my theory. In the sentence. "Let us represent in fig. 1 an ideal condition," . . . I have clearly indicated that my diagram has only an ideal significance, in order to explain my theory in the simplest and briefest manner.

In discussing my aeroplane (see p. 262), Mr. Chanute questions the formulæ used by me. I have based my calculations on the formulæ of Lilienthal and Professor Wellner, as well as on my own experience with flying models. My model of the "Aeroveloce," on the occasion of my last experimental lecture in 1892, was carefully measured and weighed by my fellow-aviators. An account of these experiments will be found in No. 708 of the *Zeitschrift für Luftschiffahrt*, Berlin, 1892, and in the *Neue Freie Presse* of Vienna, No. 9,829, January 6, 1892.

Even when we succeed in actually compassing artificial flight, there will probably be no agreement among aviators concerning aerodynamic formulæ. I have had some remarkable experiences in this direction. According to formulæ used until quite recently, my model, in proportion to the size of its wings, should not weigh more than 8 to 18 grams in order to fly at all with a velocity of 4 meters. As a matter of fact, it weighs 245 grams, and actually flies and rises. Isn't that queer? Still more droll is the fact that my models flew as long ago as 1879 and 1880, and were shown publicly in Vienna before the Chamber of Commerce, the Railway Club, the Aeronautical Society and other large bodies, with perfect success; and that, notwithstanding this, almost to the present day many theorists calculate the "lift" according to antiquated formulæ, by which neither the birds nor any artificial apparatus could ever be made to fly.

Mr. Chanute knows very well what contradictions prevail on the subject. I will not argue any further whether, in calculating the "drift," an angle of 3° or of 6° should be assumed in my aeroplane machine, but I will pass at once to the more important point—that is, to the resistance of the "spars, posts, braces, etc." This I neglected to introduce in the calculation of my machine, and thus drew upon myself a well-deserved correction.

The "spars, posts, braces, etc.," which carry the car, shafting, journals, etc., and connect these with the wings, are so arranged that a part of them are shielded from the direct wind by the wings; the parts which remain exposed to the wind will aggregate about 50 meters in length; these ties, struts, etc., will consist of "Mannesman" steel tubes of about 1¼ millimeters wall thickness, pressed nearly flat; they are hollow elliptic steel tubes placed with the sharp edge to the wind, being 35 millimeters in breadth and 5 millimeters in thickness in the middle part; they will sustain a tension of 3,000 kilograms, and weigh eight-tenths of a kilogram per meter. The wind resistance of this cross-section is reduced to

one-fifth by the pointed elliptic form (see fig. 1). Thus the resistance of these "spars, posts, braces, etc.," will be: $W^{iv} = 50 \times 0.005 \times 100 \times \frac{1}{8} \times \frac{1}{5} = 0.63$ kilograms, and not 16.67 kilograms, as Mr. Chanute erroneously assumes.

This is for a velocity of 10 meters per second, which, for an aeroplane, must be considered a minimum. For practical work we must endeavor to reach a velocity of 30 meters per second. At this last velocity the resistance of the car and the framework alone would be 172.5 kilograms, according to Mr. Chanute, and the necessary work $1,725 \times 30 = 5,175$ kilogrameters per second. Allowing 50 per cent. for the efficiency of the screw, $2 \times 5,175 = 10,350$ kilograms = 138 H.P. would be necessary to simply overcome the horizontal resistance of the apparatus.

It will be clear to every aviator that structures producing so great a resistance would be totally impracticable in artificial flight. I am willing to admit, however, that my unintentional omission in describing the details of my apparatus was the main cause of this misunderstanding. WILLIAM KRESS.

INDEX.